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VOL. II.—30TH YEAR. SYDNEY, SATURDAY, DECEMBER 25, 1943. No. 26.

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The Sir Richard Stawell Oration.[1]

THE MOSQUITO: A TEACHER OF MEDICINE.

By S. F. McDONALD, M.D. (Melbourne), F.R.C.P., F.R.A.C.P.,
Brisbane.

SOME fifteen years ago, largely on my suggestion, Sir Richard Stawell was invited by the Queensland Branch of our Association to come to Queensland and deliver the Bancroft Memorial Lecture—the highest mark of appreciation we could offer. I little thought then that I in my turn should be asked to come to Melbourne and deliver and address founded in his honour. The honour rather overwhelms me—the more so when I consider the names of my predecessors. But I ask you to remember that there is a war on, and that, since the best are not available, you must satisfy yourselves with an austerity speaker.

Richard Rawden Stawell, in whose memory this annual address was founded by certain people who loved him, was born in 1864, the son of Sir William Stawell, Chief Justice of Victoria. He resembled his father in appearance and in mental brilliance, and I have been told also in his precise and deliberate manner of speech. He qualified at the University of Melbourne in 1888, was resident medical officer at the Melbourne Hospital and the Children's Hospital, at both of which he was to become honorary physician. For thirty years as physician, teacher and consultant he held an outstanding position in Melbourne. In 1929 he was knighted, and by death in 1935 he was robbed of the two greatest prizes in medicine—presidency of the annual meeting of the Parent Association held in Melbourne, and the Fellowship, *honoris causa*, of the Royal College of Physicians.

[1] Delivered at a meeting of the Victorian Branch of the British Medical Association on October 6, 1943.

As a student, he was taught by men who had seen the first antiseptics; before he died he was to hear of the first sulphanilamides. As a student, he still heard miasma and paludism spoken of as realities; before his death two of his pupils were doing the work which would elucidate "Q" fever.

Sixty years ago, a grateful poet dedicated a book of poems in words which are still an inspiration. "There are men and classes of men", wrote Stevenson from his sick-bed at Bournemouth, "that stand above the common herd; the soldier, the sailor and the shepherd not infrequently; the artist rarely; rarelier still the clergyman; the physician almost as a rule. He is the flower, such as it is, of our civilisation, and when that stage of man is done with and only remembered to be marvelled at in history, he will be thought to have shared as little as any in the defects of the period, and most notably have exhibited the virtues of the race. Generosity he has—such as is possible to those who practise an art, never to those who drive a trade, discretion tested by a hundred secrets; tact tried in a thousand embarrassments; and what are more important, Heraclean cheerfulness and courage. So it is that he brings air and cheer into the sick room, and often enough—though not so often as he wishes, brings healing."

The more I look back over the years which separate me today from that anxious morning when I made my first appearance in "Dicky's" class at the Melbourne Hospital, the more do I realize that it might well have been Richard Rawden Stawell of whom Stevenson was writing.

One picture: An old German with a hopeless gastric cancer sat in the dingy out-patient room of the "Old Melbourne", facing "Dick", pouring out his symptoms in a torrent of German gutterals. "Dick", with his kindly grave face, answered him in German, consoling and cheering, finally writing down the prescription that was the least part of the treatment. Then said he to us: "That poor old man has a cancer of the stomach—it is quite hopeless, but I do what I can. I hope you do not mind my talking to him in German; he says it is hard to talk English when he is in pain, and I think it makes him

happier to talk his own language." How Stevenson would have appreciated that, or Henley, who wrote of Lister:

> His wise rare smile is rich with certainties,
> And seems in all his patients to compel
> Such love and faith as failure cannot quell.

I have said that a student made his first entry with trepidation. It was well known that the teaching in "Dicky's" class was of the best; but the standards demanded of the student were equally high. He was expected to know much of his subject and be ready to display his knowledge. Questions and rebukes were liberally scattered, rarely there was a word of praise; but then the grateful recipient went out into the open air, feeling that life had little more to give him. For great as was "Dicky" as a clinician, he was greater as a teacher. He was of that school which derived on one hand from the great clinicians of the eighties or nineties, but was young enough to be aware of the new lights of the laboratory workers. With C. J. Martin as his guide, he was led into the ways of biochemistry and bacteriology. In his house in Spring Street was a small laboratory, its equipment chosen by Martin—a workshop in which he lamented he had too little time to work. He might make every use of the latest scientific ideas, but it was of necessity through the hands of others; he himself was still too much the master of the physical sign and the bedside observation, ever to allow himself to become enslaved by the mere technician.

During his visit to Brisbane, in 1929, he gave a lecture on the diagnosis of pulmonary tuberculosis—a lecture which, unfortunately, was never published. It was at a time when the radiological diagnosis of pulmonary tuberculosis was being pushed to the exclusion of all else. He met this by insisting that tuberculosis was a disease and not a shadow—that if it were claimed that a certain "fan" of quite considerable extent meant tuberculosis, then there must be some basis for this "fan", discoverable *post mortem*. His words were much criticized then; but today the "fan" is never mentioned, and its author's name is forgotten. Indeed, he insisted that today the study of physical signs and symptoms is more important than ever, because the newer methods place earlier diagnosis in our hands, and yet we must still appreciate what changes we can detect in the patient by ordinary clinical means.

Thus he had no patience with the man who, feeling an irregular pulse, needed an electrocardiogram to distinguish between auricular fibrillation and a frequency of extrasystoles; nor could he tolerate those who said that elaborate history and examination were no longer necessary in gastro-intestinal disease, because the X rays and test meal examinations could tell us everything. So, too, his simple clinical aphorism that "in nervous disease the nature of the onset determines the nature of the lesion" is of more value to the average doctor than knowledge of the colloidal gold test.

But with all his insistence on the older methods of teaching, perhaps his greatest characteristic was his constant study of all that was newest in medicine and science. Let us take the Bancroft Memorial Lecture which he delivered in 1929. From a man of his clinical experience many expected a bread-and-butter clinical lecture. Instead he chose a new and profound subject—not one in which he was very greatly concerned, except as it might serve to clear up some of the problems with which he was daily struggling, but yet a subject which he felt to be of immense importance in the new ideas which spring from its study. After one had heard, or still better read, that lecture, there was little excuse for ignorance of the functions and diseases of the pituitary gland. It covered a wide new field, involving extensive reading and hard work to bring within the confines of an hour's lecture. Yet he succeeded in the task and satisfied his audience completely, saving one man who complained that certain recent work had been omitted. Inquiry proved that this work had appeared only two weeks before in an obscure journal.

It was often said: "What a pity it is that 'Dicky' didn't write more." It was—for he had an excellent gift for exposition, and as his Bancroft Memorial Lecture showed, a capacity for assembling scattered material and welding it into a homogeneous discussion. But in truth his students and his house physicians were his books—a much less enduring monument than bronze or stone, but far more lasting than much of the vast output of medical writing which today clogs and hampers our attempts at knowledge.

Like Newbolt's Ionicus:

> Beyond the book his teaching sped—
> He left on whom he taught the trace
> Of Kinship with the deathless dead.

So he left his mark on us as we strove to copy his habits of thought; but above all he inspired us with his enthusiasm for scientific truth. I have said that Stawell was one of those who made medicine an art and not a trade. He made it something more—he made it part of science. He tried to make us realize that medicine was not a mere matter of earning a livelihood—something more even that curing the sick; it was the pursuit of knowledge and scientific truth.

To every genuine worker in any walk of science, no matter how humble, there comes a time, it may be only a moment, only a glimpse lost and gone again, of realization that he is not working alone—that he is working on a huge task in company with unnumbered others. The mediaeval craftsman who carved a gargoyle in a hidden corner must have had the same feeling, as today has the riveter well and truly heading his rivet in the depths of a ship's hull. So the scientific drudge laboriously tallying the hairs on the thorax of a mosquito, the minor astronomer calculating the cycle of some distant star, and the doctor honestly and faithfully noting and observing symptoms in the most humdrum bread-and-butter case—are brother worshippers in the great temple of science. They are united by one bond—truth—and to them denial of scientific truth is the sin against the Holy Ghost. It is a standard of late conception and development; it may have begun with the Greeks; but it began to take its certain way upward with the seventeenth century scientists and the founders of the Royal Society. From then on it has developed—the study of objective truth. In our own time it is threatened by authoritarian rulers on the one hand and the glib orators of the market place on the other.

Stawell insisted on this truth in all he taught; at once that blazing eye and bitter tongue were turned on the luckless sloven who had "thought that so and so", or still worse, invented signs which were not present.

I believe that all of us who practise medicine at one time or another see this vision of the temple of scientific truth; some, like Stawell, have it before their eyes almost all their lives; to others it is but a glimpse and gone again. Even Mr. Bob Sawyer may have had it at times, perhaps when his friend dilated on the splendid operation it would be if Slasher did it. What can we do to ensure that we doctors get not only glimpses, but the constant clear vision? It is not incompatible with busy general practice; Mackenzie and Embley could keep this grip among lodge patients; Manson in China, Ross in India were able to keep it; but how many others of less stout heart have fallen by the wayside? Like McAndrew in his engine room:

> An' by Thy Grace I had the Light to see my duty plain.
> Light on the engine-room—no more—bright as our carbons burn.
> I've lost it since a thousand times, but never past return.

Overwork has been the blinding agent for some, easy success for others, seeking after false gods for yet others.

If the new plan for medicine is to be anything more than a sham, it must be designed to encourage, not necessarily research, but even more important, honest thinking, and the pursuit of objective truth. Nowhere during the past sixty years has this pursuit of objective truth, by workers toiling merely to know, yielded greater rewards than in the study of the insect transmission of disease. It began with Manson and Bancroft, and even today it is far from finished.

It was this enthusiasm for medicine as a science that made Bancroft so attractive to Stawell, and made the introduction to the lecture so great a tribute, not merely

to a lucky discoverer, but to the all-too-rare physician who was also a scientist. For Stawell felt that the great advances in medicine have been made by those who work steadily forward asking why? how?—and it was just such a type that was Bancroft. He, too, had what Stawell had and held so precious—the capacity to appreciate new ideas. So when his friend Rowlands called Bancroft's attention to Manson's discovery of the microfilariæ, his attitude was one of neither rejection nor acceptance. He looked for himself, he, too, found the parasites in the blood and in the mosquito as Manson had done; but he went further and quite independently found what Manson had not found, the adult worm. From Manson, too, he received all the help and encouragement which that great worker was always so ready to give; there was no jealousy that another worker was sharing his inspiration and extending his work. Perhaps, too, Bancroft realized how this study of mosquitoes opened an entirely new window on infection, and how much the study of the mosquito was to alter our modern concepts of disease transmission.

In infection by the worm "filaria", or as it is now called "Wuchereria", the adult remains stationary in a cyst in the lymphatic areas and does little harm. The young, on the other hand, before settling down to full development, may cause blocks in the lymphatic system, resulting in the intractable, disagreeable and finally fatal diseases of elephantiasis and chyluria.

It is rather striking how modest Manson was over his own discovery of the mosquito's part in the transmission of the filaria. Indeed, if you read one of the old editions of Manson's "Tropical Medicine", you are puzzled why he ever touched the mosquito at all. For that, indeed, you must turn to his son-in-law's account—and I cannot do better than quote Dr. Manson-Bahr at length.[1]

> In the summer of 1877 Manson began to reflect upon the destiny of the embryo filariæ: what becomes of them all? How and where do they undergo the development that permits them and fits them to take up with another host? He calculated that in the vessels of certain patients there might exist at any moment more than two million embryos. Obviously they all cannot develop in their present host without killing him and thereby frustrating their own persistence as a species; therefore, like the eggs and embryos of other parasites which live in the interior of their hosts, they must escape from their present host and then, either in a free environment, or (more probably) in the tissues of some intermediate host or hosts, grow into larval or pre-adult forms which, either in dirty water, or encysted in the flesh of the intermediate host, must be ingested by their final host, man. Undoubtedly the embryos are voided occasionally, particularly in cases of chyluria; but as their usual range is the blood, Manson concluded that they must escape from the blood by the intermediation of some common bloodsucking insect, and that that insect will be the intermediate host required. Of common bloodsucking insects there are fleas, bugs, lice, mosquitoes and sandflies. Manson excluded the first three because, as cosmopolitan insects, they could spread the filaria all over the world, whereas filariasis was prevalent only in warm countries; he selected mosquitoes because, as he then thought, their geographical range corresponded with that of the disease. (Our ignorance, like our indiscretion, sometimes serves us well; in 1877 Manson, like most of his contemporaries, did not know that the mosquito, far from being restricted to warm countries, utters forth its dolorous voice from pole to pole almost.) With female mosquitoes, therefore, he made the experiments referred to in the China Customs Medical Reports for September, 1877, and communicated, through Cobbold, to the Linnean Society in 1878.[2] In this communication he describes how he fed mosquitoes upon a man whose blood swarmed with embryo filariæ; how, subsequently, he found embryos in the stomach of the mosquitoes in such numbers (a hundred-and-twenty in one particular instance) as to make him think that the insect must in some mechanical way "select" them from the blood; how most of the embryos suffered digestion, and how those that escaped digestion proceeded to develop in the mosquito; how he followed the development up to the fourth or fifth day, when many of the mosquitoes died; how a few mosquitoes lived on until the fifth or sixth day, and how in four of these he found what he believed to be the final larval stage of the filaria. He describes this larva, its comparatively large size, its structure in part, its "boring" or "piercing" papillæ in penetrating the tissues of man and escaping from the mosquito, its activity, and the fact that "it is by no means inconvenienced by the water in which it has just been immersed". Finally, he states his opinion that it is undoubtedly the larva of Filaria bancrofti, "equipped for independent life and ready to quit its nurse the mosquito", and predicts that its next course will be to free itself from the mosquito when the insect dies (as he then supposed it did) in the water where it has deposited its eggs, and then, coming into "contact with the tissues of man" and "either piercing the integuments, or . . . being swallowed", will "work its way through the alimentary canal to its final resting-place".

There are two points to which I would call attention: the first is the almost flawless inductive reasoning when Manson was working with his own facts; the other point is the errors and long-delayed advance—of more than twenty years in fact—through his dependence on the work of others. One wonders what that "unknown reviewer", whose criticism in 1883 was so clear, knew of mosquitoes and their life history, when on reviewing Manson's paper he suggested the real solution. It is a most interesting commentary on a saying of the late Sir John Rose Bradford: "There are no useless facts in Science—we never know when the most obscure and abstract piece of scientific knowledge may not be of the utmost importance to somebody engaged on a much bigger task."[1]

Equally apposite is Faraday's answer to those who demanded what use was his discovery of electromagnetism: "What use is a baby?" Had there been in existence a complete life-history of the mosquito, available at the time of Manson's discovery, it is certain that the true history not only of filaria, but of malaria too, might have been known long before 1899. Even so, that discovery of adult filaria and the entrance of the embryo into the stomach of the mosquito, not to be digested, but to develop a stage further, opened to Manson a new world. Already the dual habitat of parasites had been shown to be a constant feature in their lives. It was true that in the case of the greater parasites like the hydatid tapeworms both hosts were mammals; but the guinea-worm's life had been worked out by Fedschenko, and here the two hosts were a mammal and a minute crustacean. So Manson went on by analogy to believe that filaria was transmitted in the same way—that in some way either the young filaria entered water from the living mosquito, or that moribund mosquitoes fell into the water, and the embryos escaped. The exact reasons which led him to adopt this theory need not detain us; but he evidently held this theory simply as a working hypothesis, and was quite prepared to drop it as soon as a better was produced.

This better theory came from the Bancrofts, for in the early eighties T. L. Bancroft had joined his father Joseph in the study of medicine and general natural history—a combination which included filarial infection. In the nineties T. L. Bancroft began to work on filaria, work in which Manson became much interested; he encouraged the new worker and placed all possible help at his disposal.

[1] Dr. Manson-Bahr hesitates to give Bancroft any credit beyond successfully confirming Manson's observation and his own finding of the adult worm. Certainly, however, it is the tradition in Queensland that Bancroft himself believed that the mosquito had much to do with the transmission of the filaria, and that when opportunity offered, his son took up the work again in the nineties. My chief source for this statement was the late Dr. E. S. Jackson, who knew both the Bancrofts well.

[2] Incidentally, since this was written, I have come across a very charming account by Manson himself in Sir Rupert Boyce's "Mosquito or Man?", page 33. It will repay reading, though it could not replace Dr. Manson-Bahr's account.

[1] An excellent example of this is the cultivation of living tissues outside the body, which was begun in 1910 by Harrison. Among the first, if not the first, to succeed in this field in Australia was a colleague of Stawell's at the Children's Hospital, Dr. W. Atkinson Wood. In 1913 I can remember his demonstrating his cultures to his residents. We thought it very clever and interesting—something above pond-hunting and a little below bacteriology. We little guessed that he was forging the crude beginnings of a weapon which Dr. Burnet was to adapt to the practical study of the Rickettsia.

In 1899 T. L. Bancroft (Joseph had died in 1894) made a final contribution, in which he put forward the view that the filaria, having developed within the mosquito, escaped down its proboscis into the new host, and he described correctly the path the filaria pursues in the mosquito's anatomy. Manson acknowledged this remarkable piece of work in his usual appreciative fashion, despite the fact that it wrecked his own theory.

It was not by then such a very revolutionary theory, for Ross had come into the field with his demonstration of the transmission of the malarial parasite by Anopheles, and Theobold Smith in 1890 had worked out the role of the tick in carrying Texas fever. But actually it was the initial work of Manson that laid the foundation of both discoveries.

Of the two, of course, the malaria parasite was a great deal more important in its effect on man. Great areas of the earth were made almost uninhabitable by the malarial fevers which raged there—great enterprises like the Panama Canal failed, not from mechanical difficulties, but from disease, while generals from Sennacherib to Chatham had watched their armies melt away under its irresistible attack. Many theories had been advanced as to cause. The one most commonly held was that of paludism—a miasma rising from nearby marshy areas. Common observation showed that ague and marshes were commonly associated, either in the temperate clime of Romney Marsh or in the moist jungles of West Africa. Observation, too, showed that with the draining of the marshes ague disappeared; as it was said in Romney Marsh, "the Bailiff of the Marshes broke his neck in a dike", "the Bailiff" being the local name for malaria.

But what was it in the marshes that caused malaria? Dr. Culpeper, indeed, in 1651, had a reply handy—"in the conjunction of the cold and wet of the marshes with the cold and wet humours of the body under the influence of that cold planet the moon"; but though his great contemporary, Sydenham, left the astrologers behind, he could still only talk of seasons, changes in the blood, constitutions and so on, and had no conception of an infection.

But later on the importance of malaria in armies, navies and colonies gave a great impetus to investigation, with unfortunately but little result, except that to the miasma theory was added the drinking-water theory. Even Livingstone subscribed to this view, though a little doubtfully, and we shall see that Manson himself long held to it.

Instances were given in which the crews of ships which received their water from malarious seaports contracted the disease, while others that did not take in water escaped. Yet it was known, too, that even in these malarious areas it was possible to ward off the disease—and a puzzling weapon against malaria was found to be a mosquito net!

Thus, to quote only one example, there was the experience of Sir Samuel Baker in 1865. He and his wife were pushing on to complete Speke and Grant's discovery of the Nile Sources, but lost most of their baggage on the way; among the rest were their mosquito nets, and it was to this loss that Baker ascribed their severe sufferings from malaria. A learned reviewer of the book, while accepting the statement, gravely discussed the ways in which a mosquito net might control "effluvia".

Even so great a clinician as Trousseau never has a hint of the cause. When he lectured to his class in 1857 he gave them an account of marsh fevers as complete and vivid as we could ask for today. Occasionally he slipped in some symptom which had nothing to do with malaria, but he omitted very little. He gave, for instance, an account of the cerebral form which leaves little to be desired, and he plainly differentiated malaria from typhoid fever. He also knew a great deal about several methods of treatment. But when we come to pathology, he speaks a language which we do not understand, beginning with a definition of a diathesis which seems to apply not at all, and containing the remarkable sentence that intermittence "belongs essentially to the organism (that is, the patient) and not to the action on it of an external cause".

Always there is the assumption as a matter of well-known fact that the cause is the effluvium from marshes; so malaria occurs in Paris following the new sewerage diggings, it occurs among men walking across a bridge in Rome into a breeze blowing from the marshes, and in the Crimea it "had no cause other than the great upturning of earth necessitated by the siege works at Sebastopol".

Today, with the key in our hands, we see how all these happenings click into place; to Trousseau and his colleagues it was a mystery. Actually the malaria parasite had been seen, for Trousseau devotes much time to the work of Frerichs. This observer had demonstrated that in the cerebral form of malaria the capillaries were packed with pigment—pigment which we now know was in the malaria parasites which were blocking the vessels. Frerichs suggested that this was an embolic process, the pigment being derived from red cells broken down in spleen and liver. Trousseau tears this theory to pieces; but he can substitute for it nothing better than "a sanguineous raptus in the nerve centres and their envelopes".

But actually, before Trousseau was wandering between diathesis and miasmata, two men, Nott of Mobile in 1848, and Beauperthuy in Venezuela in 1854, were blaming the mosquito. The latter especially went into the matter in considerable detail, holding that both malaria and yellow fever were produced by mosquitoes injecting poisons when they bit, and that marshes were dangerous only in so far as they supplied a breeding place for mosquitoes.

Trousseau demonstrated his malaria patients in 1857. He died in 1865, as great a clinician on his own deathbed as he had been at the *Hôtel Dieu*, and the world was to wait fifteen years before the cause of malaria was demonstrated by Laveran. But those fifteen years were years of medical revolution. Pasteur began his work with yeasts in 1865, went on to pebrine, chicken cholera and anthrax, and was beginning his work on protective inoculation; his rival Koch had enunciated his three postulates and grown organisms in pure culture; while Lister had applied their discoveries to found modern surgery.

So it was to a world prepared for infections and micro-organisms that a number of workers presented their discovery of the malaria bacillus. Fungi and bacilli alone or in combination with terrestrial or atmospheric changes were confidently described as the responsible agents. It was a splendid illustration of the wisdom of Pasteur's warning: "Be very careful when you are looking for a thing or you will be sure to find it." Even the Italian workers of the early eighties, when confronted with Laveran's preparations, considered them artefacts and held strongly to one of the many alleged bacterial agents.

Though other observers—notably Frerichs in 1861, or Delafield in 1872—had certainly seen the pigmented parasites, it was Alphonse Laveran, a French army surgeon, who recognized them as the cause of the disease, on November 6, 1880. He actually saw the flagellate stage developed on the slide from "certain pigmented hyaline bodies". Here we have the first recognition of pigment as the clue to the malarial body. In the pursuit of the parasite this pigment becomes the paper of the paper-chase; yet it is only the blood pigment altered by the parasite's life processes; it is an excrement like those masses of fur and bones which some carnivorous birds reject after a meal—and unwittingly guide the naturalist to their nests.

But the reception of Laveran's work was by no means gracious. Golgi and his Italian fellow workers at first regarded as artefacts the "oscillatoria" (for so Laveran called the flagellate bodies that he saw), and many others denied that the organisms were the cause of the disease. This was natural enough, for Koch's postulates of constant presence, cultivation on artificial media and reproduction of the disease by inoculation of the culture, were already accepted; but in Laveran's discovery two factors were lacking.

We must remember, too, the mechanical difficulties under which these pioneers laboured. Laveran used a dry lens giving a magnification of 400 to 500 diameters—a much poorer weapon than the "high power" of the ordinary student's microscope. Abbé and Zeiss produced their first

oil immersion lens in 1877, but it was first used by the Italians only in 1884, when they saw amœboid movement in the debated body contained in the red corpuscles. Not only were these lenses inferior, but the methods of preparation were indifferent. Today a third-year student with a little teaching can turn out stained blood films in which the recognition of the parasite becomes almost easy.

This application began with borax-methylene blue about 1884. In 1890 came the capricious Romanowsky stain, which Leishman modified in 1901 to become the daily weapon of the malaria hunter. (Not least of the benefits conferred by these improved methods of recognition is the power of early treatment. No longer with Trousseau do we have to wait for a series of attacks to be sure of our diagnosis, but in half an hour the laboratory will tell us to go ahead with our quinine.)

Though Manson himself had first seen the parasite only in 1892 at the Seamen's Hospital, yet he worked busily, and soon he had seen Laveran's phenomenon of "exflagellation", what we now know to be the production of the male elements in the sexual part of the mosquito cycle. The flagellate bodies develop only after the malarial parasite is drawn from the patient's body, and to Manson they at once recalled the release of the filarial embryo from its sheath.

> In 1894, and again in 1896, I formulated a definite hypothesis on the subject. Being a parasite, the germ of malaria, to keep in existence as a species, must pass from host to host; in other words, must at some time have an extracorporeal life. From the fact that the flagellated body does not come into existence until the blood has left the blood vessels—that is, until it is outside the body—I concluded that the function, then unknown, of the flagellum lay outside the human body, and that the flagellated body was the first phase of the extracorporeal life of the malaria parasite. As the parasite whilst in the circulation is always enclosed in a blood corpuscle, and is therefore incapable of leaving the body by its own efforts, and as it is never, so far as known, extruded in the excreta, I concluded that it is removed from the circulation by some bloodsucking animal, most probably by some suctorial insect common in the haunts of malaria. This bloodsucker I believed to be the mosquito, an insect whose habits seemed adapted for such a purpose, and whose distribution conformed to the well-ascertained habits of malaria. Further, basing my argument on what I had shown to be the fact in the case of Filaria Bancrofti, and on the peculiarities of the distribution of malaria, I reasoned that only particular species of mosquito were capable of subserving particular species of malaria parasite.

But though Manson saw the parasites in the freshly drawn blood—saw them produce the flagellate forms, and believed that these were taken up by the mosquito—he still turned as he had done with filaria to water-borne infection from the mosquito; the parasites either escaped from the mosquito as the guinea-worm larva does from the crustacean, or were taken in by the ingestion of dead infected mosquitoes in drinking water. He still held the view that the mosquito died immediately after laying her eggs, and hence did not bite again. It was not the least of T. L. Bancroft's discoveries that he independently disproved this, and showed by proper care that mosquitoes could live up to seventy days in captivity and lay eggs in several batches.

But Manson not only saw the parasites of malaria himself, but he demonstrated them to others—among them, a genius who was to carry on and complete his work, Ronald Ross. Ross, indeed, at first did not believe in the parasite of Laveran, and it was only in 1894 in London that he actually first saw a parasite under Manson's demonstration. He was convinced; others were not, for even in frank malaria parasites may be present in the general circulation only for a few hours, and at other times are invisible. One of Ross's worst humiliations was his attempt to demonstrate the plasmodium to a class of students and to his own director-general; he failed.

I do not propose to retell the story of Ross's labours and final success—of the days when he sweated in the heat of Secunderabad, when every experiment proved a failure; of how Ross dissected mosquito after mosquito, searching, as he put it, for a parasite which he would not recognize in a mosquito organ he could not foresee—a parasite whose size was to that of the mosquito as a half-crown to a hippopotamus. One may recall, however, that great day, August 20, 1897, when the first Anopheles came into his hands. On that day in their stomachs he saw the tell-tale black pigment in entirely new forms of the parasite. (One experiment must be mentioned, as it seemed to confirm the drinking-water theory. A few drachms of water containing dead malarial infected mosquitoes were given to a volunteer, who later developed a fever. It was a splendid example of the badly conducted experiment giving a "false positive".)

From then on, in spite of official difficulties and malaria-free stations, Ross's work progressed to its triumphant conclusion—to be reported as a scientific fact by Manson at the British Medical Association annual meeting of 1898. But that did not silence all the scoffers, though the work was abundantly backed up by Grassi and his fellow Italians.

In 1897 G. W. McCallum had shown the sexual cycle of the malaria in the mosquito—how the flagellate bodies were the male elements which pierced the rounded, pigmented female bodies, so fertilizing them and starting off the new asexual cycle. This discovery closed a gap in Ross's findings. Ross had actually seen this happen and had not grasped its meaning.

What completed the proof beyond any question were Manson's two experiments. In the first of these, mosquitoes infected with malaria were sent from Italy to London. In London they bit two men, who by no chance could ever have contracted malaria previously. In normal time these volunteers developed malaria. Next a party went to Italy, lived in security from mosquitoes in a bad malarial district, and did not contract malaria. Grassi had conducted parallel experiments with equal success; the jig-saw was complete.

Not the least of Manson's claims to greatness is his handling of Ross. This triple genius—doctor, mathematician and poet—was at times a difficult problem. It was obvious that his superiors took little interest in his work, and his proud nature was bitterly resentful of official neglect. Manson, with his London influence, was able to get this corrected; remembering that "poets are kittle cattle", he avoided hurting Ross's feelings, and at the same time persuaded the heads of the Indian Medical Service that Ross was worth encouraging. To Ross's credit, even in his most fretful moods, he never failed to acknowledge his debt to Manson.

But together this triple band of workers—Ross in India, Grassi in Italy, Manson in London—had produced an unassailable answer to the problem of malaria transmission; the mosquito had come into its own as a factor in the human struggle for existence, and dragged with it an ever-increasing throng of other insects, who were found to be guilty in greater or less degree of the transmission of disease. Not only this, but these diseases include the deadliest of the world's pestilences—plague, typhus fever, malaria, sleeping sickness and yellow fever. While the "vectors"—flea, louse, mosquito and tsetse fly—are all various forms of insects, the infecting parasites themselves range from the highly developed worm Wuchereria of filariasis, through protozoal trypanosome or plasmodium, to the bacillus of plague and the Rickettsia of typhus fever.[1]

[1] Medicine has for some reason adopted the word "vector" to describe an insect carrier of disease, why I do not know. "Carrier", of course, is already applied to human beings who carry and spread pathogenic organisms with no damage to their own health. The word "porter", however, might well be used instead of "vector". It was, I vaguely remember, proposed many years ago for those insects which, like flies and cockroaches, merely carry infecting agents mechanically; but actually there is no sharp line between the different ways of carrying disease. Meantime "vector" is applied alike to the mosquito in which the plasmodium of malaria undergoes development and to the tick in which the *Rickettsia burneti* of "Q" fever does not. The adoption of "vector" in preference to "porter" is, I fear, merely a manifestation of the medical scientist's love for "boss-words"—a love he, among many others, shares with Mark Twain's agent Harris and the late Jim Pinkerton.

Thus, of all the great pestilences of which we have any real knowledge, only three groups are not insect transmitted. Of these, the first group is that of the respiratory diseases, infection of which is conveyed in droplets of moisture scattered by sneezing and coughing. Influenza, pneumonic plague, measles, whooping cough, with their secondary pneumonias, cerebro-spinal meningitis and in its small scale tuberculosis, are in this group. In the next group come the excreta-borne infections—cholera, dysentery and food poisoning. The last group contains the contagious infections—small-pox, chicken-pox, syphilis (a true pestilence among virgin populations) and leprosy. But the diseases of even two of these groups may be spread by insects—typhoid fever by the "fæcal feeding filthy-footed fly", while the cockroach (and of late even the industrious ant) has been shown capable of carrying tubercle bacilli after a meal of infected sputum.

The military importance of pestilence is obvious enough, and the British Army has had its share. It began at Agincourt with dysentery, in Walcheren there was malaria, in the West Indies yellow fever, in India cholera, in South Africa typhoid fever, in France trench fever, at Gallipoli dysentery, and everywhere influenza. Experience bears out the soundness of Zinsser's statement: "Soldiers have rarely won wars. They more often mop up after the barrage of epidemics." There are even some who say that for the Owen Stanley Range victory, too little credit was given to those silent fifth columnists, the germs of malaria and dysentery.

But once we have gained our knowledge, it is of little use to say glibly: "Now we know that these diseases are transmitted by insects we can deal with them." On the contrary, we are only beginning. Let us consider malaria for a moment. We have seen that it was only his falling almost by chance on the Anopheles that led Ross to his great find; but a little further study leads to the perplexing discovery that all Anopheles are not equally culpable, and that not all Anopheles have the same habits of life—habits which make them more or less vulnerable to attack.

The ravages of malaria among our troops in certain areas in New Guinea are well known. The Anopheles concerned was the *Anopheles punctulatus*, or even more, its variety *moluccensis*. These mosquitoes breed in sunny water, so that scrub-clearing will increase the breeding places. It is also stated definitely that both feed by night, but by day also if the sky is overcast, so that protective clothing must be provided at all times. Actually control has been most successful; but meantime, in another area to which malarial-infected troops were being moved, large numbers of Anopheles were found, breeding under conditions to make control most difficult. Much anxiety was felt till it was found that this was a species of Anopheles which did not carry malaria—at least not in Australia. And here is another point: it is possible that a mosquito may be a disease carrier in one area and not in another, and much time may be spent destroying a comparatively harmless form while the real culprits escape. In all this work, therefore, we come back to the same problem—the determination of exact knowledge. It is common knowledge that this search is still going on at high pressure in this country, and that the original worker responsible for it is another of Stawell's distinguished former students—Colonel Neil Hamilton Fairley.

Two tropical diseases, both with a high incidence in a non-immune population, are yellow fever and dengue fever—the one associated with a high mortality, the other with a low one. Both are carried by a mosquito common to tropical and subtropical Australia—the *Aëdes ægypti* (originally called *Stegomyia fasciata*). Here there is little to be gained by dealing with large pools and sluggish streams as with the *Anopheles punctulatus*. The *Aëdes* is a domestic mosquito, and it is in small collections of domestic water in any hollow from a flower vase to a hoof-print that the larva will be found. In Queensland last year we had quite a brisk epidemic of dengue fever; one important breeding place was the partly filled air-raid shelter. House tanks lacking gauze protection and garden puddles were well populated, and one small epidemic was distributed by a colony which grew up in a limb hole four feet from the ground. It was the mosquito-minded T. L. Bancroft who, during the great Queensland epidemic of 1905, first suggested this carrier after careful experiment. So far no case of yellow fever has reached this country, but this disease is known to be on the march; it is slowly making its way across Africa, and its final jump between Africa and Australia may well be by aeroplane.

Where attack has been possible, as at Panama and in Brazil, the *Aëdes ægypti* has been beaten and yellow fever with it; we hope it may be possible to repeat this success in this country with dengue fever—a disease important, not from its killing, but from its temporarily crippling powers.

But without exact knowledge of the mosquito no attack is successful and the disease flourishes. So the epidemic entomologist has come into his own, and the skilled observer who recognizes adult, larva and pupa is worth hundreds of ignorant enthusiasts.

The organisms causing yellow fever and dengue fever are viruses, and so far they are unisolated—unlike the Rickettsia of typhus fever; this last, too, is not mosquito-borne, but louse-borne, and the louse, unlike the carrier of dengue, dies even more surely than the human victim. So far we have had no true louse-borne typhus fever in this country, though North Africa and central Europe have it. But we have its milder relatives—the mild flea-borne murine typhus and the more serious scrub typhus, borne by neither flea nor louse, but by the larva of a mite.

Another relation of typhus fever may have been the trench fever of the last war, louse-borne, and now strangely disappeared; no cases appeared even in the 1939-1940 winter in areas where in the 1914-1918 winters there were tens of thousands.

There are two other insect-borne diseases of great interest—one a great pestilence, the other a minor affliction. Much of what we know about their transmission we owe to the work of Australians. The first is bubonic plague. There is little need to remind you of its importance—the Black Death and the Great Plague of London are events in European history; but till the beginning of this century nothing was known for certain of its transmission. Mr. Pepys knew as much as did our most expert workers, till in India and Australia the matter was investigated and settled. In India, very early in this century, Liston, with whom there worked an Australian, Elkington (still serving the Commonwealth), first pointed strongly to the rat and flea as the source and channel of the disease.[1]

But it remained for Tidswell and Ashburton Thompson in Sydney to settle the matter finally during the New South Wales epidemic of 1900-1906. They made plain the sequence: black rat dying of plague, his flea forsaking him and seeking for preference another black rat, but for lack of something better choosing a human host. Meanwhile in the flea's "straining-organ" (as Elkington called it in the paper he read before this Branch almost exactly forty years ago), the bacilli taken from the plague-infected rat are caught, but live on to multiply and choke the outlet of the flea's forestomach. When the next meal is taken in, this forestomach overfills, and like an overfed baby, the flea regurgitates the excess back through its sucking-tube into the tissues of its new host, rat or man. No better means of inoculation could be adopted.

This story of insect transmission of disease may be completed by that of "Q" fever. This is a rickettsial disease, whose life history and transmission have been worked out by two brilliant graduates of this medical school, both also students under Sir Richard Stawell—Dr. F. M. Burnet and Dr. E. H. Derrick. "Q" fever is found almost entirely among abattoir and dairy workers in Queensland. It is primarily a disease of the bandicoot or similar small bush animal, and is carried from one to another of these by ticks. To cattle also one species of these ticks (*Ixodes*) can carry the disease, probably by biting and injection. The cattle in turn infect their own numerous cattle ticks. These ticks do not bite men; but

[1] J. S. C. Elkington was of our university, though not a graduate. His father was for many years professor of law.

partly in their crushed bodies, but even more in their fæces, is the infecting virus present, to be dried, powdered and inhaled by the dairyman or worker at the abattoirs.

As I have said, we satisfy ourselves by the words, "these diseases are insect transmitted"; but in scarcely any two is the mode of transmission the same.

The younger Bancroft showed that the filarial embryo made its way through the structures surrounding the proboscis of the mosquito into the proboscis, by which it passed down into the human skin, penetrating through the puncture made by the mosquito. The sporozoites of malaria pass—by what agency we do not know—into the salivary glands of the mosquito, and thence are carried down the proboscis in the flowing anticoagulant saliva which the mosquito injects.

On the other hand, it is believed that the yellow fever virus can make its way through the unbroken skin. The earlier workers with trench fever believed that its virus was carried by the louse and injected by biting; but Byam showed that this only rarely occurred, and that the most common method was by scratch inoculation of louse fæces containing the Rickettsia. This is a path vastly different from that followed by the immature filaria.

All these details and much more must be known if we are to attempt any real control of the insect-borne diseases. We can learn them only by the efforts of our scientific research workers. But scientific workers fall into two very different groups. In the first are those people to whom scientific work is, as Dr. Burnet said in his Bancroft Memorial Lecture, a pleasant escape from reality. They undertake it with no great fervour for any benefit that they may confer on mankind; it is simply a job, and a most absorbing job, done with care and thoroughness, but with little more. Such research workers are being trained in every university, and you can get them, not quite at two a penny, but certainly for no more than one need pay a first-class cook or salesman. Many are women to whom the test tube and microscope have taken the place of the knitting needle and embroidery frame, for consistent and absorbing work. Such workers are the blessing of modern research; they make the daily blood counts and routine tests in every case, and leave the director free for planning, and for that most difficult and important part of the research, thinking.

It is this director who must have the exceptional qualities that make the original thinker and worker. We are accustomed, for instance, to hear the discovery of the "sulpha" group quoted as an instance of German originality in science; actually, of course, it was not an original plan, but merely the application—carried out, indeed, with immense care and thoroughness by certain workers—of a plan originally suggested years before by Ehrlich, the original thinker. It is sometimes said: "But nowadays when the elaboration of apparatus and methods is so great, the necessity for these master minds is less." On the contrary, with the overwhelming amount of minor work that is going on, it takes a master to pick out the wood from the trees. Volumes are published every year reporting "research" in cancer. I think you could count on one hand the really significant advances of the last twenty years. Nor does the best work always come out of the most elaborately equipped workshops. Dr. William Castle proudly described his brilliant investigations into the anæmias as being done with a kitchen-equipped laboratory; though Reed and Lazear had no microscope that would have shown them a yellow fever organism if they had isolated it, they settled the transmission of yellow fever. To come nearer home, Stawell's great friend Sir Charles Martin prided himself on being able to make with a tinsmith's tools his own apparatus from rubber tubing and odd tin canisters.

The original worker, above all, must possess that rare flair for picking out the essential from the non-essential. We look at many advances in medicine and we wonder why they were not made before, it all seems so obvious. (But Hamilton Russell, another friend of Stawell's, used to say: "The most common fault in medicine is an inability to appreciate the obvious.") Nearly all the great advances in medicine could have been made much sooner, had people grasped the significance of certain well-known facts; only comparatively rarely have they occurred as an entirely new conception—the result of the application of completely new methods. Mosquito transmission of malaria, for instance, could have been proved in Sir Samuel Baker's day, had anyone made the "obvious" deduction.

Many people had tried to grow Rickettsia on living tissue, but these living tissues died and the Rickettsia with them. Dr. Burnet made the great step forward by using embryonic tissues which went on living; but Dr. Burnet is an original worker. Many of us in Queensland had seen "Q" fever cases, puzzled over their failure to fit into our known diseases; but it was Dr. Derrick who set to work, investigated the disease and demonstrated the cause.

Now, these original people, as opposed to routine workers, are rare. Australia in the past has produced singularly many of them; but, sad to say, it has lost them. Sometimes, like Sir C. J. Martin, they are strangers, brought here as little-known men, to depart great ones; sometimes they are natives who have spent most of their working lives here, like the Floreys or Sir Thomas Dunhill, but we lose them just the same. Worst of all is the fact that we have such people in our midst and let them wear out in routine jobs, when they are capable of much higher things. (It is pleasing to remember that Stawell was among the few to recognize the worth of one of these.) At times there has been much talk of prohibiting the export of Australian stud rams; the export of first-class brains is a far more serious menace.

Two other difficulties which beset the life of the absorbed scientific worker are the familial and the economic. These I can best illustrate by quotations: John Hunter, leaving an experiment to see a patient, says: "Damn that guinea!" Madame Pasteur, on her silver wedding morning, writes to her daughter: "Your father this morning rose, dressed, and went to his laboratory exactly as he has done every day since he married me twenty-five years ago."

What is being done to encourage the appearance of these original workers? Is our present medical course designed and administered to this effect—or is there too great a tendency to encourage merely routine thinking and assimilation of examination-passing formulæ? Such a system is much easier both for pupil and teacher; but it does not make for original thinking, and it was not Stawell's way. "Let us find out", he would say, and the examination of the sick child became an exciting voyage of discovery.

The born "research worker" is not always an easy man to handle. I have told of Manson's skilful handling of Ross. It is not by any means easy for the older generation to forsake beliefs of a lifetime and accept the work of younger men. Lewis does not always find a Mackenzie, and McNee is too apt to encounter a Sloggett.[1] It is our duty as Australians to be sure that our really original workers are found and used. I do not know how; frankly, I dread the dead hand of authority, and believe rather in a system which should lay stress on independence, criticism, and that state of intellectual truculence which produced such figures as Paré and Florence Nightingale, Gorgas and Freud. It may be that many investigations will run into dead ends under a free system; but again, to quote John Rose Bradford, "a well conducted enquiry leading to negative results may be of far more value to science than an apparently triumphal success—built on a foundation of bad work". For it means, not only that we must give the investigator opportunities to work, but we must train our controlling pundits to recognize new ideas.[2]

[1] Mackenzie cordially welcomed Lewis's demonstration of auricular fibrillation, which displaced his own theory of "nodal rhythm". Sir Arthur Sloggett, as Director of Medical Services, refused to allow Captain (now Professor) J. W. McNee to complete his study of trench fever. For this unedifying story see "The Official History of the Australian Army Medical Services in the War of 1914-1918", Volume III, page 246.

[2] A surprising example is Sir David Bruce's half-contemptuous reference to "that almost mythical Genus the Rickettsia" in the introduction to Byam's "Trench Fever".

Not the least sign of real scientific greatness is the capacity to consider new ideas—often heterodox—in an entirely objective fashion. Popular ideas, too, are not always wrong, though the superior mind scoffs at them; but Jenner was not above taking a hint from a dairy maid, David Bruce believed African savages, and Theobold Smith learned from cattle drovers. To the official mind—especially the lay official mind—such a capacity is too often suspect. Stawell was fond of reminding us that progress does not always come in straight lines, and he would quote Meredith, "On the World's Advance":

> Judge mildly the tasked world, and disincline
> To brand it, for it bears a heavy pack—
> You have perchance observed the inebriate's track.
> At night when he has quitted the inn-sign
> He plays diversions on the homeward line.
> Still that way bent—albeit his legs are slack.
> Our world's advance presents that figure on a flat—
> The way of worms.

How far we can make that drunkard's homeward course straighter by better teaching, by organization and endowment, is, I believe, a great matter for debate; but in settling the problem, I believe that we may learn much from all that has happened since Manson first found his filarial embryos in the mosquito, and Bancroft the adult worm in the inguinal glands of man.

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THE KLINE REACTION OF NAURUAN LEPERS AND NON-LEPERS.

By L. A. WINDSOR-MCLEAN, M.B., B.S. (Adelaide), D.T.M. (Sydney),

Brisbane.

DURING the time that I was medical officer in Nauru for the British Phosphate Commissioners, it occurred to me that it would be interesting to test the Kline reactions of the Nauruan lepers. I had in mind the fact that it has been claimed that leprosy *per se* may cause positive flocculation or complement fixation reactions for syphilis. Unfortunately, it was impossible in an isolated spot such as Nauru to supplement the Kline flocculation test with the Wassermann complement fixation test, for the simple reason that it was impossible to obtain complement. However, as regards the specificity and sensitivity of the Kline flocculation test as compared with complement deviation reactions, it is probably safe to accept the findings of the United States Public Health Service in the series of reprints from *Venereal Disease Information* that there is "relatively equal value . . . of the efficient complement fixation tests and efficient flocculation tests as applied to either blood or spinal fluid specimens".

Venereal Disease in Nauru.

It was at first thought that it might be possible to rule out the possibility of syphilitic taint in a spot so isolated as Nauru. However, inquiry soon showed that this would not be possible, for gonorrhœa has existed and exists amongst the natives, and if gonorrhœa has been present one cannot definitely exclude syphilis. However, in 1925 Dew reported as follows: "Venereal diseases are rare. Two cases of gonorrhœa were treated . . . both of whom claimed to have contracted the disease many years ago. In a survey of the entire population . . . not a single case was detected. No syphilis, primary, secondary, tertiary or congenital, has been observed."

So far as I am able to ascertain, the position is still much the same. An occasional case of gonorrhœa occurs, and there is no definite history of syphilis—this, notwithstanding the fact that a certain amount of casual cohabitation undoubtedly takes place between the Chinese indentured labourers and Nauruan females. But one must also remember that the Chinese labourer is subjected to a thorough physical examination, both before leaving Hongkong and again on his arrival in Nauru, and so far as my own personal experience goes, in fifteen months I have not encountered a case of either gonorrhœa or syphilis amongst the Chinese labourers. It would, however, have been an interesting experiment with some bearing on the subject now under discussion to make a Kline survey of the Chinese labourers. Unfortunately there was not time.

To sum up, then: syphilitic taint is remotely possible but not particularly likely amongst present-day Nauruans. On the other hand, it will be noted that in a few instances some of the subjects who reacted to the Kline test give definite history of having had gonorrhœa.

Yaws in Nauru.

Yaws is a more difficult problem. Dew reports that yaws is practically non-existent, which seems to indicate that some did exist in 1925. I think that one could safely state that no clinical yaws was present at the time this survey was carried out. Probably there had been some in past years.

The Present Investigation.

Blood was taken from the median cubital vein of all the inmates of the leper station. Whenever a positive reaction to the Kline test was obtained, the test was repeated. Fifty-two inmates of the leper station were so tested. As controls, 57 ordinary patients from the Nauruan Hospital were also subjected to the Kline test.

Technique.

Blood was collected into sterile bottles and the serum was separated and pipetted off. It was inactivated by being heated to 56° C. for half an hour in a water bath, and 0·05 cubic centimetre of inactivated serum was mixed with 0·0075 cubic centimetre of Kline's antigen on a slide within a wax ring. The Kline antigen used was that made by the Commonwealth Serum Laboratories and was prepared with the usual precautions according to the following formula:

0·85 cubic centimetre of distilled water.
1·25 cubic centimetres of a 1% solution of cholesterol in absolute ethyl alcohol.
0·1 cubic centimetre of antigen.
2·2 cubic centimetres of a 0·85% solution of sodium chloride.

The serum and antigen were mixed on the slide with a wooden stick; mixing was continued by rocking the slide, and the result was read under the low power of the microscope at such time as the "positive" control was showing marked flocculation while none was visible in the "negative" control.

Results.

Table I sets out some of the results.

Thus, of the inmates of the leper station seven out of 52, or 13·5%, gave a positive reaction to the Kline test.

Among the Nauruan inmates of leper stations who did not react to the Kline test, there were 30 males and 15 females. The ages of the males ranged between ten and sixty-three years, and those of the female patients between twelve and sixty-five years.

Among the controls who reacted to the Kline test, there were five men, ranging in age from sixteen to forty-eight years, and three women, ranging in age from eighteen to sixty years. Two of the men gave a history of yaws in youth and one a history of gonorrhœa. One of the women had suffered from yaws twenty to twenty-five years earlier. In the remaining cases the history did not suggest yaws or syphilis. One female and one male had a history suggestive of leprosy.

Among the controls who failed to react to the Kline test, there were 29 males and 20 females. The ages of the men ranged from sixteen to fifty-eight years and those of the women from thirteen to fifty-eight years. Six of the male and four of the female controls had previously suffered from leprosy or were suffering from leprosy at the time of the investigation. In no instance was there a history suggestive of syphilis or yaws.

In the assessment of the value of a positive reaction to the Kline test amongst the controls, two of the positive reactors and ten of those who failed to react are excluded, because some history of leprosy or suspicion of leprosy renders them of no value as controls. This leaves six positive reactions amongst 45 controls, or 13·3%.

In the consideration of the results it has been thought wise to exclude the possible effect that syphilis and yaws may have had upon the Kline reactions, because the histories and evidence of these diseases are rather vague. It has therefore been assumed that such effects of these diseases as may have been active will have been equally distributed between the leprous and non-leprous groups. It seems impossible to do otherwise.

The difference in the percentage of positive Kline reactions between the leprous (13·5%) and the non-leprous (13·3%) is 0·2% and obviously of no statistical significance; but if this is doubted, the standard error of difference deduced from the formula

$$\epsilon(D) = \pm \sqrt{\frac{P_1(100-P_1)}{N_1} + \frac{P_2(100-P_2)}{N_2}}$$

is ± 6·9. No one will doubt that a difference of 0·2 ± 6·9% is not statistically significant.

Thus, as far as this investigation goes, there is no evidence that leprosy causes the patient to react to the Kline test.

Summary.

The responses to the Kline test of 52 leprous Nauruans and 45 non-leprous Nauruans have been investigated.

There is no statistically significant difference between the groups. Therefore, this investigation has produced no evidence that active leprosy tends to produce a positive reaction to the Kline test.

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I have to thank the former Government Medical Officer of Nauru, Dr. T. M. Clouston, and the Assistant Government Medical Officer, Dr. B. Quin, for allowing me access to the Leper Station, for themselves collecting some of the specimens of blood used in this investigation, and for providing the clinical details concerning the subjects.

TABLE I.

Nauruan Inmates of the Leper Station who Reacted to the Kline Test.

Number and Name.	Sex.	Age. (Years.)	History of Leprosy.	Remarks.
1. Dekanea[1] ..	Male.	34	Diagnosis made 1921: N_3-C_3.[2] Admitted leper station, 1921.	No relevant history. No suggestion of syphilis or yaws.
2. Goduwa ..	Male.	35	Diagnosis made and patient admitted to leper station, 1924. Released on parole, 1937, and readmitted to station. Now N_1-C_3.	Gonorrhœa as a youth. No clinical suggestion of syphilis or yaws.
3. Emilia ..	Female.	30	Diagnosis made and patient admitted to leper station, 1922. Has steadily become worse and shown poor resistance. Now N_3-C_3. Family history bad as regards leprosy.	No history to suggest syphilis or yaws. No miscarriages and all children healthy; had seven children in eight years.
4. Tsimauw	Male.	19	Developed leprosy in 1933 and admitted to leper station. Now N_1-C_1.	Born in Gilbert Islands and no early history available. Nothing clinically to suggest syphilis or yaws.
5. Rirubwe ..	Male.	10	Born in leper station, 1929, of leprous parents. Removed at birth. No available history of further contact. Developed leprosy 1934, and admitted to station. Now N_1-C_1 and showing good resistance and response.	Healthy child apart from leprosy. No stigmata of syphilis and no signs of yaws.
6. Eiyamangir	Female.	25	Diagnosis made and patient admitted to leper station in 1921. Released on parole, 1932. Treated at out-patient clinic, but relapsed and was readmitted to station, 1935. Now N_1-C_3. Not showing much resistance. Poor response to treatment.	No suggestion of syphilis or yaws.
7. Robert D.	Male.	10	Diagnosis made and patient admitted to leper station, 1937. Shows good resistance. Now N_1-C_1. Family history bad as regards leprosy. May possibly be released soon.[3]	No stigmata of syphilis and no signs of yaws.

[1] Died of leprosy on July 9, 1939.
[2] The symbols "N_1", "N_3", "N_2" indicate the degree of neural involvement and the symbols "C_1", "C_3", "C_2" the degree of cutaneous involvement.
[3] The investigation was made in 1939.

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"Report of the Administration of Nauru during 1924", Appendix A, page 23.

Reviews.

A SYMPOSIUM ON WAR MEDICINE.

"WAR MEDICINE: A SYMPOSIUM", as its name implies, is a collection of papers written by well-known British and American authors, most of whom hold high rank in the respective services.[1] The subjects range over a wide field of war medicine and surgery and must interest and profit the reader. While the book is essentially practical in outlook, the theoretical aspect has not been neglected and in all the articles due emphasis has been placed on physiological and pathological principles. The book could readily form the basis for a series of post-graduate lectures to service medical officers.

The work of the regimental medical officer has not been overlooked, and the opening chapters contain much information useful to the medical officer working in a forward area, or one dealing with early casualties. The need for the early recognition and treatment of shock is stressed so that operative procedures may be undertaken more safely. Colonel Max Page in an article on "Surgical Principles in Divisional Units" gives some good advice on the early treatment of casualties and mentions the importance of early evacuation and operation. Experienced medical officers will note with approval that the Thomas splint is strongly recommended for the transport of patients suffering from wounds of the lower limbs. In another article, "The Healing of Wounds", it is suggested that each soldier should carry a five gramme ampoule of sulphanilamide powder as well as tablets of the same drug, a wise provision for immediate use, locally and orally, should he be wounded. This chapter is an excellent contribution and contains sound matter of interest to all surgeons on the principles of wound healing and the pathological processes involved. In passing it might be mentioned, however, that there is no reference to the flavines as antiseptics.

With such contributors as Wakeley, Horrax, Ivy, Grey Turner, Sellors, Gordon-Taylor, Whitby, to mention but a few, the expectation that such subjects as burns, wounds of the brain, jaws, heart, abdomen and thorax, as well as shock, would be discussed in a masterly manner is fulfilled, and throughout the book this general excellence of matter and treatment is maintained.

The importance of a knowledge of the physiology of the plasma proteins and the part they play in the prevention and treatment of shock and allied conditions is stressed in a well-written and lucid article by Frank B. Marsh. This article and those on the diagnosis and treatment of shock and the use of blood substitutes deal with this most important subject in a comprehensive manner.

Articles of special interest are those dealing with the wounding mechanism of high-velocity missiles, blast and concussion, principles and physiology of aviation and high-altitude flying, deep sea diving and sudden compression injuries of the abdomen at sea. They all reveal the necessity for still further and continued research, observation and investigation in these newer fields of war medicine—made all the more necessary by the total nature of modern warfare.

Such ordinary but important subjects as the nutrition of the soldier, painful feet, tinea, malingering and shamming which often receive scant notice in the text-books receive adequate attention and will be noted appreciatively by service medical officers.

The article on malingering deserves some special mention. The writer discusses the difficulties encountered in this aspect of work and at the same time realizes the need for fairness to the nation, the services and the man himself. Most or all medical officers experienced in "boarding" will agree with the opinion that there is a very small degree of deliberate malingering in the services. The author draws attention to the fact that "it is equally malingery, and of equal import, if a person endeavours to feign health in order thereby to gain some advantage to which, in truth, he is not entitled". He, too, feels that more psychiatrists are needed to detect the early psychoses and writes: "It appears to many observers that the segregation of any and all enrollees with the least suspicion of psychiatric defect for a prolonged observation would, in the end, be a wise course." Again, those who have had experience on medical boards will be inclined to agree.

The book is well produced, except for the diagrams and illustrations, which are very few in number and poorly reproduced. There is a large bibliography following each article, but an index to assist quick reference would have been a welcome addition. This book can be highly recommended to service and civilian doctors alike for study and reference.

[1] "War Medicine: A Symposium", edited by Winfield Scott Pugh, M.D.; 1942. New York: Philosophical Library. 9¼" × 6", pp. 573, with illustrations.

INFANT FEEDING.

DR. BRAITHWAITE'S book on infant feeding in general practice in its second edition is a small, simply written book which contains much useful information.[1] The first two chapters are devoted to the consideration of breast feeding, the second being taken up with the difficulties which occur during lactation. The advantages to both mother and child of natural feeding are emphasized. In the following chapters various ways of using fresh cow's milk, dried milk and condensed milk are described. In the section dealing with nutritional disorders the author follows the classifications of Czerny and Finkelstein and gives well chosen examples of methods he has used in treatment. The feeding of the premature infant is shortly dealt with and there is a chapter on deficiency diseases. As H. C. Cameron, who writes a foreword, states: "A book by a general practitioner written for general practitioners is likely to have one advantage, that writer and readers have a similar outlook and would lay stress upon the same aspects of the subject." In this small book there is much information which should prove helpful to the doctor who wishes to accept his due responsibilities for the babies in his practice.

MEDICAL ETHICS.

IN producing a brief and essentially practical work on medical ethics, Major-General Downes has rendered the profession in Australia a service.[2] The book is based on a series of lectures given by the late A. V. M. Anderson, of Melbourne. As the author points out in the preface, the work is intended to be merely a plain statement of the present understanding of the principles of medical ethics. Whilst intended primarily for the Victorian medical practitioner, the book will serve in the main as a useful reference for any member of the profession in Australia who, when faced with unfamiliar circumstances, is anxious to do the right thing.

There are a few points on which everyone will not accept the author's views, for example, on obtaining patients and on the nationalization of medicine which is really a medico-political question. The book as a whole, however, reflects the accepted views on the duties of the medical practitioner to his colleagues, his patients and the State.

[1] "Infant Feeding in General Practice", by J. Vernon Braithwaite, M.D., F.R.C.P., with a foreword by H. C. Cameron, M.A., M.D., F.R.C.P.; Second Edition; 1943. Bristol: John Wright and Sons Limited; London: Simpkin Marshall (1941) Limited. 7¼" × 5", pp. 176. Price: 7s. 6d. net.

[2] "Medical Ethics", by Rupert M. Downes, C.M.G., V.D., M.D., M.S., F.R.A.C.S.; 1942. Melbourne: W. Ramsay (Surgical) Proprietary Limited. 8¼" × 5¼", pp. 78. Price: 5s.

The Medical Journal of Australia

SATURDAY, DECEMBER 25, 1943.

All articles submitted for publication in this journal should be typed with double or treble spacing. Carbon copies should not be sent. Authors are requested to avoid the use of abbreviations and not to underline either words or phrases.

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SOME ASPECTS OF THE DIPHTHERIA PROBLEM.

WHAT is known as the diphtheria problem is an enormous subject. It concerns the epidemiology of the disease, its clinical manifestations, bacteriological considerations, treatment, prevention, the carrier state and so on. In addition it includes the obligations of the State, the medical profession, and the relatives and associates of affected persons. To discuss these in detail in this place would be impossible and indeed unnecessary, first because most of the facts are known to practitioners of medicine, and secondly because they are readily obtainable in text-books by those who wish to refresh their memories. Recent happenings in at least one centre have, however, made it desirable to refer to certain aspects of isolation and carriers and of institutional treatment.

The control of diphtheria today has been made possible by the introduction of the Schick test and by active immunization. Persons who have a natural immunity, and those who have acquired immunity by clinical or subclinical infection or by repeated exposure to small doses of the Klebbs-Löffler bacillus which do not give rise to a manifest infection, can be discovered by their failure to react to the Schick test. Persons who do react and are therefore to be regarded as susceptible to infection can be immunized by the injection of immunizing agents. Some misconceptions are current in regard to immunization against diphtheria. In the first place active immunization is never likely to give complete protection to a community—it will, if properly carried out, reduce the number of infections till they are almost negligible, but cases of diphtheria will always occur from time to time. In the second place, if the incidence of diphtheria and its attendant mortality in a community are to be reduced, it is necessary that approximately 70% of the children under fifteen years of age shall be immunized and that special attention shall be paid to children of the pre-school age—60% of these at least should receive prophylactic treatment. Some interesting figures on this aspect are quoted in a recent report by W. T. Russell, published by the Medical Research Council of Great Britain.[1] W. W. Lee is reported as having laid stress on the importance of protecting the pre-school child. Lee stated that in Philadelphia it was estimated that 40% of children under five years of age, between 80% and 85% of those between the ages of five and nine years, and between 60% and 65% of those between ten and fourteen years of age had been immunized, with the result that the mortality rates for 1930 were "below the expected rates for 1930 at distances far beyond the range of pure chance and therefore attributable in large degree to diphtheria immunization". Russell shows that the total death rate from diphtheria in American cities is determined by the contribution made by the children under five years of age. In New York City it was claimed that in 1940 60% of the children under five years of age were immunized. During that year the expected death rate from diphtheria was 24·0 per 100,000; the actual death rate was 0·8 per 100,000. Russell points out that active immunization is not the sole determinant in the control of the death rate, because mortality can be and has been reduced by the administration of antitoxin in the treatment of the disease. He then shows from New York data that sufficient children had not been protected in 1927 to prevent an epidemic in that year. But the two epidemics which, according to the cycle of epidemics, should have taken place between that year and 1940, did not occur. There is also "very substantial evidence" that by artificially immunizing the child population, the public health authorities in Toronto have practically eliminated diphtheria from their city. On the question of the immunization of a population and the eradication of diphtheria it may be concluded that the degree of freedom of a community from infection is an indication of the efficiency of the public health authority.

In regard to the isolation and treatment of patients it must suffice to state that generally speaking all sufferers from diphtheria are better treated in hospital. It has been stated by a leading Australian authority that for a practitioner skilled in diphtheria treatment no disease is easier to manage in a private home. At the same time many practitioners are said to be afraid of diphtheria and serious errors can easily be made if the practitioner has not a good deal of experience behind him. Unfortunately all the centres in Australia are not provided with suitable isolation hospitals and the practitioner's difficulty may be considerable. The decision as to which patients should be sent to hospital is not always easy. In a statement prepared recently by the Council of the Fever Group of the Society of Medical Officers of Health in Great Britain[2] the view is expressed that there is a tendency to place undue reliance on the result of swab examination—the bacteriologist cannot distinguish between active diphtheria and the carrier state and his report must be interpreted in the light of the clinical findings. In most cases no difficulty will be experienced. Sometimes a test for virulence of diphtheria organisms will have to be carried out and perhaps a Schick test also made; sometimes a search will have to be made for hæmolytic streptococci or organisms of the Vincent type. It should be

[1] "The Epidemiology of Diphtheria during the Last Forty Years", by W. T. Russell, Medical Research Council of the Privy Council, Special Report Series, Number 247; 1943. London: His Majesty's Stationery Office. 9¼" × 6", pp. 52. Price: 1s. net.

[2] *British Medical Journal*, August 7, 1943, page 177.

pointed out that an infectious diseases hospital has as one of its functions the observation of patients with suspicious infections and the making of a diagnosis. In this regard the Fever Group of medical officers states, and rightly, that the public should be taught to realize that "the doctor who sends a doubtful case to hospital for investigation is doing better by his patient than the doctor who waits for the result of swab examination".

Another dictum from the statement of the Fever Group needs emphasis: "An individual whose throat or nose is swabbed because of contact with an established case but who has no symptoms must not be classed as a case of diphtheria, notified as such, or sent to hospital merely on account of a positive finding." When doubt arises, the swabbing should be examined by culture methods. Occasionally one failure to grow *Corynebacterium diphtheriæ* will be sufficient, but as a rule two swabbings taken at intervals of forty-eight hours should be incubated. In suspicious cases—those in which rhinorrhœa is present, for example—three or four cultures may have to be attempted. Once a diagnosis of a dangerous carrier state is made, the patient should be sent to hospital or isolated in a satisfactory manner. There should be no loophole by which a patient dangerous to others can avoid isolation. In cases of difficulty the practitioner will expect to be supported by the health authority in any stand that he may take. If support is not forthcoming, adequate grounds for action taken by the health authorities should be stated and examination by a responsible departmental medical officer should be made to justify the action.

In any discussion on carriers certain factors have to be considered. These include the possibility that the carrier may infect other persons, the natural susceptibility of the community at risk (compare, for example, an infants' home and a factory), and the state of immunity of that community. Three types of persons may be considered in connexion with the carrier state. First there are persons in good health, not recent contacts, in whom diphtheria organisms are discovered. In most cases the organisms are found to be non-virulent, and this group may be ignored. The second type may be described as temporary carriers. They are recent contacts, they seem perfectly well and their throats look healthy. If diphtheria does not become manifest within a few days the bacteria quickly disappear. The application of the Schick test at the time of swabbing will show whether these persons are likely to develop diphtheria or not. The third and most dangerous type comprises persons who are up and about; they have stigmata of diphtheria such as rhinorrhœa, sores, cervical adenitis and so on, and diphtheria organisms are present in throat and nose. Most of these persons have unhealthy tonsils, and most of them also do not react to the Schick test. If symptoms have been present for only a few days a reaction to the Schick test may be obtained. In regard to this group of persons determined measures should be taken. They are suitable for admission to hospital.

The last aspect of the subject to which reference will be made is that of measures for dealing with infection in institutions. Clearly conditions vary in regard to the age of inmates of institutions and also in regard to the permanency of the residence of the inmates. In an institution with a permanent population safety will be achieved if immunization is carried out regularly and on all new entrants. This is a matter which should not be undertaken only at the instigation of those in control of the institution. It is the duty of the public health authority to show its efficiency by inducing those in charge to have regular immunization carried out. The occurrence of diphtheria in an institution that is properly controlled will occasion no anxiety. On the other hand the occurrence of diphtheria in an institution whose inmates have not been immunized will be difficult to handle. In any case, whether the institution is alive to its responsibilities or not, the health authority should automatically investigate the occurrence of diphtheria within its walls. In regard to an institution whose population changes rapidly, there is not time for active immunization to be carried out and the use of an isolation ward becomes necessary together with application of the Schick test. In certain circumstances active immunization may be begun in the hope that when the child moves on to another *milieu* the process will be completed. This procedure is adopted at the Children's Hospital, Brisbane. All children on admission, if their health permits, are given their first immunizing injections, so that if they remain for more than a few weeks the process is complete. If they are discharged before all necessary injections have been given, they are sent by the hospital authorities to the city health office in Brisbane and immunization is completed. Most difficulty in institutions occurs with children who are admitted with unhealthy noses and throats and with rhinorrhœa. These children are generally convalescent carriers and are best treated in an infectious diseases hospital unless they belong to institutions having isolation blocks where any necessary treatment can be carried out. Sometimes they derive benefit from antitoxin treatment, but in many instances removal of tonsils and adenoids is necessary.

Reference has been made to only one or two of the more important features of the diphtheria problem, and in regard to these much more might be added. The plea is really for guidance and stimulus from the departmental health authority and also for adequate provision of isolation and treatment facilities. Medical practitioners by and large are prepared to meet their obligations and will cooperate with an authority that gives a confident lead.

Current Comment.

INTRAMEDULLARY ADMINISTRATION OF FLUIDS.

MOST practitioners have been slow to adopt the intramedullary route for the administration of fluids. There would seem to be two reasons for the lack of popularity of this method: firstly, a failure to appreciate the simplicity of the method, and secondly, a fear of infecting the bone marrow and initiating osteomyelitis. This procedure was introduced by L. M. Tocantins[1] and consists of the insertion of a sternal puncture needle through the outer table of the sternum; the stilette is removed and then the needle is connected to the infusion apparatus. If there is any doubt that the needle has been correctly inserted, that is, that the point of the needle has entered the marrow, this may be confirmed by withdrawing a few drops of fluid resembling blood. The periosteum should be carefully infiltrated with a local anæsthetic solution before the sternal puncture needle is inserted. In the absence of a special needle a strong lumbar puncture

[1] *Proceedings of the Society for Experimental Biology and Medicine*, Volume XLV, 1940, page 292.

needle suffices. The introduction of the needle into the bone requires some force; but it is imperative that it should be controlled force so that the characteristic "give" is recognized when the outer table of the bone is perforated. Proficiency in this procedure comes with practice and it is soon realized how easy it is. In regard to the fear of osteomyelitis, this is obviated by an aseptic technique. The bone marrow *per se* has some powers of resistance to infection, but it is likely that it would not be able to cope with the degree of contamination that sometimes occurs owing to carelessness during the procedure of cutting down on and cannulizing a vein.

The indication for the use of the intramedullary route is the inability to give fluids by the intravenous route in cases in which the need of fluids is urgent. As R. E. Strain[1] points out, this may arise if a patient is extensively injured or burnt so that the usual sites are not available. In some cases of severe shock venospasm and stagnation of the circulation occur, so that the flow of fluids into a vein is very slow and in some cases stops completely. Occasionally in such circumstances it is possible to force the fluid along the vein with a syringe and a three-way connexion, but it is much easier in these cases to give the fluids by the intramedullary route. The veins between the sternum and the heart appear to be less affected by the venospasm than the peripheral vessels. In some of the blood dyscrasias the life of the patient depends on the facility with which repeated transfusions are given. In a case of aplastic anæmia it is a disservice to the patient, to put it mildly, to cut down on one of his precious veins and risk the occurrence of thrombosis in it. Apart from this, the intramedullary route would appear to be of considerable value for the transfusion in such a case when the direct introduction of a needle into a vein has failed. Similarly, this procedure should also have a place in the treatment of hæmophilia, for the patient with this disease has an added reason for avoiding an incision over his veins. In the hæmophiliac the proclivity to bleed is from lacerated and incised wounds rather than from punctured wounds made with a needle.

The rate of flow of fluid into the marrow is in most cases slightly slower than into a vein, but the prolonged administration of fluids into the sternum does not seem to set up a thrombophlebitis such as is sometimes seen in the peripheral veins. It has been shown by T. E. Wilson[2] that repeated sternal punctures may be performed without eliciting any evidence of reaction on the part of the marrow; and E. A. Doud and J. E. Tysell[3] have reported a case in which the infusion was given into the sternum for five days and then after a day's rest for another four days without any untoward result. Nevertheless, clotting may occur in the needle if there is delay in starting the flow of the fluid through it once the marrow has been entered. R. M. Jones[4] suggests that in such circumstances the lumen of the needle may be cleared by passing the stilette along it and by running in some saline solution with a syringe. There is not the same objection to this as there is to the indefensible practice of forcing an offending clot along a vein, as is sometimes advised when a cannula or needle becomes blocked during the intravenous administration of fluids.

Strain has recently discussed transfusion sites in patients in whom the usual situations are not available, and he includes for emergency use the lesser saphenous vein just behind the lateral malleolus, the femoral vein medial to the femoral artery below the inguinal ligament, the internal jugular vein, and the *corpus cavernosum*. Perhaps the last-mentioned route is safe, but there must always be the risk of infection. However small this risk is, it is probably sufficient to condemn this route of administration of fluids. If infection and sloughing or scarring of the *corpus cavernosum* ensued there would be more than a possibility that litigation would follow and that heavy damages would be awarded to the patient. The other routes suggested by Strain would probably not be available in those cases in which the usual sites were damaged, and in shocked patients they would exhibit the venospasm similar to that present in other veins. Even in children in whom the centres of ossification of the sternum are small, if the needle is inserted cautiously in the mid-line the procedure is devoid of danger, and in a case in which a fatality is likely unless infusions of blood or serum are given immediately, there should be no hesitation in employing the intramedullary route if the usual sites for infusion are not available or if the fluids will not flow at these sites.

[1] *The Lancet*, January 10, 1942, page 61.
[2] THE MEDICAL JOURNAL OF AUSTRALIA, May 2, 1942, page 513.
[3] *The Journal of the American Medical Association*, December 12, 1942, page 1212.
[4] *Surgery, Gynecology and Obstetrics*, May, 1943, page 587.

A SPECIAL JOURNAL FROM SWITZERLAND.

THE publisher and editor of the *Schweizerische medizinische Wochenschrift* are to be warmly congratulated on the special number of their journal which is dedicated to the International Committee of the Red Cross in grateful appreciation of its noble activities and has "*Chemotherapie*" as a supplementary title. There is a preface by the president of the Association of Swiss Doctors and a second preface by editor and publisher. No evidence of paper shortage is manifest in this fine publication which measures thirteen and a half inches by nine and a half inches, is paginated from 545 to 687 and weighs one and a half pounds. The opening article by Professor Rudolf Staehelin is on fundamental considerations in evaluating the success of a drug, but the remaining thirty articles by Swiss experts deal with sulphonamide drugs in almost cyclopædic completeness. Pneumonia, endocarditis, meningitis, children's diseases, appendicitis, trauma, gonorrhœa in male and female, tonsillitis and its sequelæ, urinary tract infections, skin complaints and pathological conditions in obstetrics, gynæcology, ophthalmology, oto-rhino-laryngology, senility and dentistry are the main topics handled and their responses to sulphonamide treatment are described. Other articles deal with the pharmacological assay, methods of estimation, changes in the blood arising from sulphonamide medication and the nomenclature and general survey of these drugs. A useful bibliography and a good index help to make this number a first-class work of reference. Just how Switzerland manages to send her medical journals abroad is a pretty question which we can well leave alone; what concerns us is that we are fortunate from time to time in finding them to hand.

MONOCULAR MYASTHENIA GRAVIS.

IT has been known for some years that the earliest symptoms of *myasthenia gravis* may appear in the eyes. An example of this was reported by A. H. Douthwaite in the *British Medical Journal*, Volume I, 1925, page 108. Transient eye symptoms also occur sometimes in *encephalitis lethargica*. In *Archives of Ophthalmology* of October, 1941, M. T. Moore reported the occurrence of monocular *myasthenia gravis* and discussed its differential diagnosis by prostigmine methylsulphate. S. S. Winton has now reported the occurrence of a similar case in a girl, aged sixteen years. The onset was sudden. The patient suffered from a moderately severe cold in the head and chest; this cleared up, and three weeks after the onset of the cold, having felt normal for ten days, she awoke one morning to find the upper left eyelid drooping and almost covering the pupil. Apart from her inability to raise the eyelid, she had no other symptoms. On the subcutaneous injection of one cubic centimetre of a one in two thousand solution of prostigmine methylsulphate, the patient was able to raise the lid. Subsequently treatment was carried out with prostigmine and guanidine; after some months the patient remained symptomless on small maintenance doses. It would be of interest to follow this patient's history over the next few years.

[1] *The Journal of the American Medical Association*, August 21, 1943.

Abstracts from Medical Literature.

PHYSIOLOGY.

Effects on Man of Severe Oxygen Lack.

S. M. HORVATH, D. B. DILL AND W. CORVIN (*The American Journal of Physiology*, March, 1943) report that schizophrenic patients have been subjected to severe anoxia over a period of several minutes either up to the point of unconsciousness or in some cases extending into unconsciousness. The following conclusions were drawn. Anoxia of severe degree produces no beneficial effects on the mental condition of this class of psychotic patients. Anoxia severe enough to produce brief periods of unconsciousness has no lasting harmful effects on the central nervous system. Respiratory stimulation by anoxia is strong and sustained even during unconsciousness. It is inferred that circulatory function is also well sustained. There is a remarkably rapid return to normal when either air or 14% oxygen is supplied. A mixture of 4·2% oxygen with nitrogen is equivalent physiologically to an altitude of about 31,000 feet, so that it should be possible to descend with an opened parachute from this height without oxygen equipment with no ill effects from anoxia.

Regional Relationships of Rate of Water Loss in Normal Adults in a Subtropical Climate.

G. E. BURCH AND W. A. SODEMAN (*The American Journal of Physiology*, March, 1943) describe some results obtained in measuring the rate of water lost by the skin in different parts of the body. Dry oxygen gas was passed through a shallow cup which was sealed to the skin by a water-soluble, non-contracting rubber cement. The water taken up by the gas was removed by refrigeration and weighed. In this study of seventeen different areas of skin in forty-six normal adults it was found that there is a marked regional variation in the rate of sweating. The most rapid rates of insensible perspiration are from the hands, feet, forehead and cheeks. The skin of the trunk, arms and legs has relatively slow rates of insensible water loss. There are marked variations in the rate of insensible water loss for the same area from subject to subject, and from time to time in the same subject. These marked variations result in overlapping of values for the various areas. Similarly, there are marked variations in the rate of sensible perspiration stimulated by a hot and humid environment. The rate of water loss increased often to a greater extent from those areas which showed little insensible sweating, when sensible sweating occurred, than from the areas which showed the largest rates of insensible water loss. There is no definite evidence of difference in the rate of insensible or sensible water loss during winter or summer months when the measurements are made under constant laboratory conditions. The rate of water loss from the skin of a subject resting quietly in bed is not materially increased when the temperature and relative humidity are increased from 75° F. and 50% respectively to 95° F. and 75%. When the temperature is further increased to 100° F., sweat literally pours. In a humid subtropical and tropical environment when heat loss is interfered with, muscular exertion should not be maintained for prolonged periods of time.

The Effect of Experimental Thyreoid Abnormalities on Appetite.

J. WARKENTIN, L. WARKENTIN AND A. C. IVY (*The American Journal of Physiology*, May, 1943) report some experiments in which normal rats and some with thyreoid abnormalities were allowed free selection and continued access to various foods. This selective feeding, when relatively pure constituents are used, will support good health and growth in most rats for a period of over one year. All food constituents should be kept in separate feeding cups; some should not be mixed together (such as yeast and salt). The diet selected by rats varies according to age, and the critical age dividing "young" and "old" rats is about four months. Older rats eat less food per 100 grammes of body weight than do young rats. Young rats select more fat and salt than do older rats, while the latter eat more protein. However, all the authors' groups have eaten much more protein than is allowed in the Osborne and Mendel diet. The alternate intake of fat and carbohydrate is so definite in some rats as to suggest that they "eat for Calories". Six rats were made hypothyreoid, six others were made hyperthyreoid and six were kept as normal controls. The thyreoidectomized rats showed a markedly greater decrease in food intake than did the normals, while the hyperthyreoid rats ate much more food than either of the other two groups. No characteristic qualitative differences in food selection were noted as a result of thyreoid abnormalities.

Effect of Tourniquets on Venous Blood Sugar Values.

W. C. LOUGHLIN, H. O. MOSENTHAL AND R. HALPERN (*The Journal of Laboratory and Clinical Medicine*, June, 1943) state the results of clinical blood sugar determinations with and without the use of a tourniquet. Without a tourniquet the venous blood sugar level, obtained at minute intervals over a six-minute period, shows no variations that cannot be ascribed to the limit of error inherent in the procedure employed. The results after the application of a venous tourniquet indicate that some adjustments occur that will cause the blood sugar to undergo fluctuations that are significant from the clinical point of view. Such changes deviate about 20 milligrammes per 100 cubic centimetres from the expected value; they generally take place within two minutes after the application of the venous tourniquet. As a rule there is a rise of the blood sugar level, though in one instance there was a marked fall. These initial variations in the venous blood sugar content are transient, and are followed by a shift in the opposite direction after the tourniquet has been in effect two or three minutes. The increase of the glucose concentration in the venous blood, while a venous tourniquet is acting, may be ascribed to the forcing of arterial blood with its higher sugar content into the occluded veins. Ebert and Stead have shown that the effect of venous tourniquets on the extremities is a reduction of the plasma volume in the blood vessels, resulting from a transudation of fluid into the surrounding tissues. Since the concentration of glucose is greater in the plasma than in whole blood, such a diminution of plasma volume would account for a lowering of the blood sugar when the amount of plasma was less and the red blood cell content was greater. These two factors, increased accumulation of arterial blood in the veins and diminished plasma volume, occurring at different intervals after the application of a venous tourniquet, would account for either a rise or a drop in the venous blood sugar level. The absence of these two influences with no tourniquet at all would explain the stabilization of the venous blood sugar under those circumstances. Venous blood sugar levels determined after the application of an arterial tourniquet are slightly more variable than those when no tourniquet is used. This may be accounted for by the phase of venous tourniquet pressure in the application of the arterial tourniquet. It is probable that if the tourniquet could be adjusted with great speed, the venous blood sugar figures derived from the use of the arterial tourniquet would duplicate those obtained without a tourniquet.

A Study of the Effect of Spontaneous Variations in Blood Pressure upon Spontaneous Variations in Volume of the Finger Tip.

C. NEUMANN (*The American Journal of Physiology*, March, 1943) states that by the simultaneous use of a plethysmograph for recording changes in volume of the tip of the left index finger and of an intraarterial manometer for obtaining synchronous readings of the blood pressure of the left radial artery, it was shown that the spontaneous variations (increase or decrease) in volume of the finger tip are not concordant with spontaneous changes in blood pressure and are present even in the absence of measurable variations in blood pressure. A few exceptions were noticed. Rises in systemic blood pressure during expiration were accompanied by variable but small increases in volume of the finger tip. Marked lowering of blood pressure accompanying cardiac systole was reflected in a decrease in volume. The rule then seems to be that variations in the volume of the finger tip usually go on independently of changes or lack of change in blood pressure, though under certain conditions there may be a transitory relationship. When present, it is manifested by an increase in volume when there is a rise in blood pressure.

Low Heart Rate in the Newborn Rat.

F. L. MARCUSE AND A. V. MOORE (*The American Journal of Physiology*, May, 1943) report the results of an experiment undertaken out of curiosity, to discover whether the heart rate of a one-day-old rat could be electrically recorded. One-day-old rats, when compared with 120-day-old (tamed) rats, were found to have a significantly lower heart rate and less fluctuation in the heart rate. Sex differences present in

the tamed, mature animals were not present in the one-day-old animals. In the first twenty-four hours of life significant changes in heart rate were observed. In a twenty-one-day period of daily heart recordings, it was observed that the heart rate increased steadily in the first eleven days of life. There was a plateau of no increase in rate from the eleventh to the twenty-first days. Sex differences manifested themselves with increasing clarity from the tenth day on.

The Origin of the Sensation of Hunger.

H. FINISTERER (according to a short-wave broadcast from Berlin on April 5, 1943, reported in *The Journal of the American Medical Association*, June 12, 1943) has successfully performed several operations in which the stomach was completely removed and the intestine sewn to the œsophagus. The patients retained the normal sensation of hunger; thus the assumption that hunger pangs originated in the stomach would appear to be false.

BIOLOGICAL CHEMISTRY.

Sulphonamides.

M. HEINEMANN (*The Journal of Clinical Investigation*, January, 1943) has estimated colorimetrically sulphanilamide, sulphathiazole, sulphapyridine and sulphadiazine after addition to defibrinated human blood. The reaction associated with the distribution of these substances is instantaneous, and independent of temperature and the presence of oxygen. Transfer of these substances occurs under suitable experimental conditions in both directions, from serum to cells and from cells to serum. Sulphonamide compounds attain equal concentrations in erythrocytes and in leucocytes. The percentages of these drugs in serum ultrafiltrates vary; they correspond to the concentrations they attain in cells and seem to determine their rate of distribution in whole blood. Of the four compounds studied, only sulphanilamide was found to be more concentrated in cells than in serum; sulphathiazole, on the other hand, reached the highest relative concentration in serum. Free passage of these compounds and varying ratios of distribution suggest that measurements of concentrations in plasma or in serum would be more informative than in whole blood. These observations also exclude the use of any of these substances for the measurements of body fluids.

Carbonic Anhydrase.

S. STEVENSON (*The Journal of Clinical Investigation*, May, 1943) has made an attempt to correlate unexplained cyanosis in newborn infants with low concentrations of carbonic anhydrase in the blood, because (*a*) it had been found that goat fœtuses were low in carbonic anhydrase, (*b*) carbonic anhydrase is found with hæmoglobin in the red cells, and (*c*) of a clinical impression that some premature and full-time newborn infants, who were doing poorly and who were exhibiting cyanosis, improved after whole blood transfusions. The concentration of carbonic anhydrase in the blood of newborn infants is less than one-half, and the concentration in premature infants is only one-quarter of that found in the blood of adults. Spontaneous changes in the blood enzyme level do not usually occur, but significant increase in the level can be accomplished by whole blood transfusions. Thirteen infants were studied who exhibited cyanosis, unexplained by recognized physical causes, and who were doing poorly. In many instances these infants showed levels of carbonic anhydrase which were significantly low. Improvement in respect to cyanosis and general condition was accompanied by a rise in blood concentration of carbonic anhydrase. The rise occurred in one infant spontaneously and was minimal. In the remaining infants, the rise followed transfusion and was significant in most instances. The findings show that many premature infants who are doing poorly and exhibit cyanosis have low levels of carbonic anhydrase. Following transfusions of adult blood, the level of carbonic anhydrase increases and clinical improvement, accompanied by a disappearance of cyanosis, follows.

Serum Lipides.

A. WINKLER *et alii* (*The Journal of Experimental Medicine*, May, 1943) report that in the dog and monkey bilateral nephrectomy or ureteral ligation results in a marked, progressive increase of total fatty acids, of free and esterified cholesterol, of phospholipid and of free fat of serum. No such changes follow unilateral nephrectomy, splenectomy or fasting. The increase after bilateral nephrectomy is not inhibited by glucose administration. A marked increase of the phospholipid and a less significant elevation in cholesterol content of the liver accompany this increase of serum lipids after bilateral nephrectomy.

Penicillin.

C. RAMMELKAMP AND C. KEEFER (*The Journal of Clinical Investigation*, May, 1943) have investigated the absorption, excretion and distribution of penicillin. Data are presented concerning the blood concentration and urinary excretion of penicillin after the administration of 5,000 to 40,000 Florey units by several routes. Intravenous injection of penicillin resulted in high initial concentration in the blood plasma which was followed by an abrupt fall. Traces of penicillin were found in the blood for 30 to 210 minutes after the injection, the length of time depending on the amount administered. The sharp fall noted in the serum concentration immediately after injection was associated with an increased excretion in the urine. The average excretion after intravenous injection was 58% of the administered dose. Penicillin was rapidly absorbed when given intramuscularly and slowly absorbed after subcutaneous injections. Excretion in the urine was rapid following intramuscular injections and delayed after subcutaneous injections. Absorption from the body cavities was delayed, and this was reflected in the slow excretion of penicillin by the kidneys. The total amount found in the urine was somewhat lower than that found following intravenous injection. Fluid aspirated from the pleural and joint cavities, twenty-two and thirteen hours after the injection, showed appreciable amounts of penicillin remaining. Administration of penicillin by enteral routes showed that absorption from the duodenum was rapid, whereas orally and rectally administered doses were poorly absorbed. These findings may be explained by the inactivating effect on penicillin of acid and *Escherichia coli*. After oral, intraduodenal and rectal administration, the average amount excreted in the urine was extremely small. In the presence of renal failure, penicillin was not excreted rapidly, and as a result, high concentrations were maintained in the blood stream after intravenous injections. Studies on the distribution of penicillin showed that the substance failed to penetrate the red cells in significant amounts. In general, the average concentration found in the erythrocytes was less than 10% of the plasma concentration. No penicillin was found in the spinal fluid, saliva or tears of subjects receiving it intravenously.

Coagulation.

H. TAGNON *et alii* (*The Journal of Clinical Investigation*, January, 1943) report on the coagulation defect in hæmophilia. The rate of dissolution of the clot obtained by the action of chloroform on hæmophilic plasma is much slower than in the case of normal plasma preparations. The rate at which chloroform plasma preparations derived from normal human plasma can cause fibrinogenolysis is much greater than that of similar preparations from hæmophilic plasma. It is concluded that the proteolytic activity of chloroform preparations of hæmophilic plasma is less than for such preparations from normal human plasma.

Creatine.

N. TIERNEY AND J. P. PETERS (*The Journal of Clinical Investigation*, July, 1943) have measured the concentrations of creatine and creatinine in the serum and urine in the post-absorptive state in normal adults and in patients of both sexes suffering from thyreoid disease. In addition, clearances were measured in similar subjects after the ingestion of one gramme of creatine. Creatine appeared in the urine only when its concentration in the serum was 0·58 milligramme *per centum* or more. In normal males it did not exceed this limit. In males the serum creatine rose little after administration of creatine, and creatinuria was minimal. In women, even if post-absorptive serum creatine was low and creatinuria lacking, administration of creatine induced a greater rise of serum creatine and more creatinuria than it did in males. In hyperthyreoidism, post-absorptive serum creatine was usually high and attended by creatinuria. Alimentary creatinæmia and creatinuria were exaggerated. Iodine therapy tended to restore these reactions to normal. Creatine is completely reabsorbed by the renal tubules when its concentration in the serum is less than 0·5 milligramme *per centum*. Above this, the clearance of creatine rises as its concentration in the serum increases. In these experiments, the creatine clearance remained always lower than the creatinine clearance. Therefore, no evidence of creatine secretion was obtained.

Medical Societies.

MELBOURNE PÆDIATRIC SOCIETY.

A MEETING of the Melbourne Pædiatric Society was held on September 8, 1943, at the Children's Hospital, Carlton, Melbourne, DR. WILFRED FORSTER, the President, in the chair.

Cyst of Submaxillary Gland.

DR. J. G. WHITAKER showed a boy, aged four years, who had had a ranula removed from the right side of the floor of the mouth at the age of six months. Three years later, because of a swelling in the neck which had developed after the first operation, the right submaxillary salivary gland was removed and its duct tied. During the six months preceding the meeting the swelling in the neck had recurred. This cystic mass was found to contain blood-stained fluid, in which Dr. Reginald Webster was able to demonstrate salivary ferments in large quantities. There was no discharge into the mouth, the submaxillary duct being quite closed. Dr. Whitaker thought that those present would be interested in the cystic change which had occurred in the remnant of the submaxillary gland. He proposed to remove it at an early date.

DR. ERIC PRICE said that he trusted Dr. Whitaker would explore the swelling. He was curious to know why aspiration should produce blood from it. Probably hæmorrhage occurred into a preformed cyst. The pathology of ranula was by no means clear. It was generally regarded as any large submucous cystic swelling in the floor of the mouth. Dr. Price said there were two types, the first of which shelled out satisfactorily. The second occupied a position to the lateral side of the tongue in the sublingual sulcus. He had been tempted to remove this type of swelling on two occasions, but not since; the swellings had proved to be dilated submaxillary ducts. Dr. Price said that now he deliberately made a fistula for them.

DR. WILFRED FORSTER said that the text-books advised the introduction of some sclerosing agent after the cyst had been opened. At operation it was a difficult matter to dissect the cysts out, and recurrences were common. In his experience, good results followed the introduction of a sclerosing solution, such as ethanolamine, and the incidence of recurrences was reduced. Dr. Forster said that he had had experience of cutting the submaxillary and parotid glands. This resulted in the formation of a fistula. He had not experienced the subsequent cystic remnant that characterized the present case. He wondered whether the original diagnosis was correct, and whether perhaps a branchial cyst existed.

Dr. Whitaker, in reply, said that hæmorrhage had apparently taken place into a preformed cyst, as clear fluid had been obtained on aspiration on a previous occasion. Dr. Price's observations were interesting. Dr. Whitaker had not experienced a cystic swelling arising in the submaxillary duct, except in those cases associated with calculus formation. Dr. Whitaker said that the two other cystic swellings beside ranula which might blast a surgeon's reputation were thyreoglossal cyst and ganglion. In this case, however, he proposed to explore the cystic swelling and remove it if possible.

Undescended Testicle.

Dr. Whitaker's second patient was a boy, aged ten years, upon whom two days previously he had performed a Torek operation for undescended testis. Two points were worth remembering in these cases. In the first place there was no need for extensive division of vessels. If the *vas deferens* and accompanying vessels were freed retroperitoneally, the testis could readily be brought down. In the second place, one should note that in a young child with an undescended testis the epididymis and testicle were separated by a long mesentery, and not fused as they were in the adult. Dr. Whitaker said that in nearly every case of undescended testis a considerable amount of "Antuitrin-S" was given by injection. In many cases it was not realized that the testis could not descend because it was ectopic—in other words, it had come out of the ring and had gone up. "Antuitrin-S" would have no effect in such cases. It should be given only when the testis was high; it was malpractice to use "Antuitrin-S" when it could not possibly influence the testicle at all. The testicles that did not descend were those that "came down and went up". Dr. Whitaker said that included in the group of boys with bilateral undescended testes was the boy who was a little overweight and a little feminine. Dr. Whitaker was anxious to know whether anyone had records of such a person producing children. He had operated on such patients, but they had not yet reached the reproductive age. It was a responsibility to know what to advise in these cases. Dr. Whitaker was interested in the factor or factors responsible for making a testicle useful in the scrotum. Temperature was thought to be important in this respect.

DR. H. C. COLVILLE said that Torek's original operation was performed in 1927. Dr. Colville had been performing the operation ever since, and had seen a large number of patients similarly treated. He therefore felt competent to express an opinion. The first point he wished to make was the great importance of correct diagnosis. At first sight, it seemed that few conditions offered less difficulty in that respect; but in fact, there were few conditions in which more mistakes were made. This arose from the fact that a large number of small boys normally wore their testes at a higher level than the scrotum. These boys were found on repeated examinations to have empty scrota. It took time to establish the fact that the testes were retracted and could be brought down into the normal position, and thus to demonstrate that the child was a normal individual. If these boys were classed as having undescended testicles, one got into a maze of wrong ideas about the whole thing. The classical instance of this was the well-known research carried out at Scotch College some years earlier. Dr. Colville had no hesitancy about discussing the work, though the author was absent from the audience, for he had thrashed out the matter with him previously. It was quite obvious that the research was based on the fallacious diagnosis suggested. The findings which emerged from the investigations could not be taken seriously—for example, that the disorder was very common (one in every nine boys), that there was no evidence of any association between undescended testis and hernia, and that nearly 100% of the subjects when followed to puberty were found to be normal. In actual fact, the reverse of these statements held good. Dr. Colville said that the genuine cases were those in which the testis occupied an ectopic or abnormal position. The outstanding point was that, whatever ectopic position the testis did occupy, it was fixed in that position, and in no case of this nature had it ever descended except after operative interference. There were therefore two classes of such cases: firstly, that of the alleged undescended testicle, which was not undescended at all, and which descended of itself; secondly, that of the ectopic, fixed testicle, which was incapable of being brought down. "Antuitrin-S" could not possibly do anything in any genuine case. It caused enlargement of the testis, and the so-called successful result consisted in the enlistment of the force of gravity in order to produce a more cosmetic effect. The treatment by "Antuitrin-S" was not justified in any case. With regard to the question whether such boys could become fathers, Dr. Colville said that the testes developed in their normal position, and there was no reason why normal spermatogenesis should not occur.

DR. ROBERT SOUTHBY said he was interested in Dr. Colville's criticism of Dr. McCutcheon's work. It was really a question of terminology. Dr. Colville referred to his cases of truly undescended testes as of ectopic testes; Dr. McCutcheon's work did not refer to his cases of ectopic testes, but coined the term "delayed testis" to describe them. He demonstrated, by yearly examinations, that in more and more of such cases the testes descended. Dr. McCutcheon himself regarded the position of the testes in these cases as physiological.

Dr. Whitaker thanked Dr. Colville for his remarks. He also felt that there were no cases calling for the use of "Antuitrin-S". His purpose in bringing the subject forward had been vindicated, and he was indebted to Dr. Colville for putting the matter so clearly and succinctly.

Pseudo-Hermaphroditism.

Dr. Whitaker's next patient was a child, aged two years, who had been presented at the last meeting of the society by Dr. Lawrence Stokes. Dr. Whitaker said he was struck by the fact that more risks could be taken in this case, since the child had comparatively little to lose. It had not yet been proved that the syndrome was attributable to adrenal disease. The pituitary was apparently normal, as also were the pelvic organs, except for the clitoris. Major Jolsen, of the United States Army, was consulted to advise on the question of perirenal inflation. After discussion, it was decided that this was unwarrantable in such a young child, and so the procedure was not carried out. Once the

decision to operate had been made, it was difficult to know what approach to make and which side to enter. In Dr. Ann McLeod's case the right kidney was palpable, and so the surgeon had an indication as to which side to explore. There was no such helpful feature in the present case. Cahill suggested a transperitoneal approach from the front; but in all his cases a tumour was palpable. Dr. Whitaker said that he made a preliminary attempt at the transperitoneal approach on an autopsy specimen; it seemed quite possible to explore the left kidney by this means, but it was a difficult matter on the right side, so Dr. Whitaker decided to approach both suprarenal glands by Young's procedure. To combat adrenal shock, the child was given adrenal cortex, and saline solution and glucose were given intravenously. "Sodium Amytal" and nitrous oxide and oxygen were used to induce anæsthesia. The operation was a simple one. The left suprarenal gland was approached first. The costo-vertebral ligaments were divided, the twelfth rib was retracted upwards and the adrenal was seen and handled. Dr. Whitaker was at a loss to explain why it did not come out when the kidney was delivered. No tumour and no hypertrophy of the gland were noticed. The other side was similarly approached and clearly visualized; no abnormality was apparent. The question then arose as to what should be done. Approximately one-half of the gland which offered the easier approach (that on the left) was removed. The operation took about one hour, and was not difficult. On leaving the operating table, the child was seen to be in a rather desperate condition. The temperature soon rose to 106° F. and the pulse became uncountable. Thanks to the efforts of Dr. Speed, the child recovered. The development of left basal congestion called for the exhibition of sulphanilamide. The site for intravenous therapy became secondarily infected, and cellulitis of the leg developed. At the time of the meeting, except for slight elevation of the temperature, and slight discharge from the leg, the child was well. Dr. Vera Krieger had again estimated the urinary androgens; these had fallen from 20 milligrammes *per centum* before the operation to 0·7 milligramme after the operation, the normal figure being one milligramme. Dr. Reginald Webster had examined sections of one-half of the biopsy specimen. He had ventured the tentative opinion that some degree of hyperplasia of the cortex was present, but said that he wished to compare the specimen with a normal gland from a child of the same age before being dogmatic. Dr. H. F. Bettinger had not yet reported on his half of the specimen. In conclusion, Dr. Whitaker said that if the male characteristics had been suppressed, the operation could be looked upon as a useful procedure.

DR. REGINALD WEBSTER said that he had examined the gland. He thought a case could be made out for hyperplasia, but wished to guard against furnishing a positive report on flimsy evidence, or just because such a report was expected or desired. The *stratum granulosum* seemed wider than normal, and examination of the epithelial cells lining the acini revealed hyperchromatism. In one place also an ectopic islet of *stratum granulosum* could be seen. Dr. Webster said he had endeavoured unsuccessfully to demonstrate the fuchsinophile reaction. Before final judgement was pronounced, it would be a wise procedure to cut two or three sections of adrenals from children of the same age and compare them carefully with the biopsy specimen obtained.

Dr. Southby said that he had noticed a report of a case in THE MEDICAL JOURNAL OF AUSTRALIA. It was a replica of the case under discussion, though the patient was older. At operation a tumour was found in the suprarenal gland, but the patient died from a post-operative suprarenal crisis.

Dr. Whitaker, in reply, stressed the fact that the removal of the adrenal was performed as a remedial measure, and not merely for research purposes.

Multiple Hydatid Cysts of the Lung.

DR. W. R. FORSTER showed a female child, aged four years, who had been admitted to hospital when one year old with bilateral congenital dislocation of the hips. On manipulation, that on the right side appeared to be easily reduced, but great difficulty was experienced on the left side. The limbs were allowed to stay in plaster casts for five or six weeks before it was decided that the position of the left hip was unsatisfactory. A further attempt was made to reduce the dislocation on the left side, and this resulted in fracture of the shaft of the femur. The late Dr. W. Kent Hughes had taught that if one could not get a hip into place, one should place it in a plaster cast and try again later. Dr. Forster held that there was a danger that fracture would result if this procedure were put into practice, as the present case had demonstrated. At the time of the meeting, the position was that, on the right side, the head of the femur was in position, but was deformed, and on the left side the head of the femur was not even in the socket.

Dr. Forster said that during the year the child was noticed to have a cough. Examination of the chest revealed diminished breath sounds and dull percussion note over the upper lobe of the right lung anteriorly, and similar signs over the upper lobe of the left lung posteriorly. X-ray examination revealed a large cyst on each side, anteriorly situated on the right side and posteriorly on the left. The cysts were regarded as hydatid cysts. Dr. Forster said it was an interesting sidelight that the child had been an in-patient at the hospital since early life. Neither the complement fixation test nor the Casoni test produced a reaction. It was possible that the case might be one of bilateral congenital cystic disease of the lungs. It remained to decide what should be done. If the cysts were hydatid cysts, the outlook was reasonably good. If the condition was an example of congenital cystic disease of the lungs, it was a great problem to know what to do.

DR. H. C. COLVILLE said that he thought the appearances and findings were suggestive of hydatid disease. The negative responses to the Casoni test and the complement fixation test need not be taken too seriously. The mode of infection was doubtful, in the case under discussion, but apparently some periods were spent outside the hospital. The condition was not confined to country dwellers. Infected dogs roamed the city streets. *Tænia echinococcus* in the intestine of a dog might be so numerous as to run into millions, and the number of ova discharged per day into many millions. So the possibilities of infection were tremendous. The radiological appearances were characteristic. From the surgical viewpoint the cyst in the left lung appeared more easy of approach. Dr. Colville thought that should be dealt with first and the right later.

DR. J. G. WHITAKER said that he wished to make three points. The first was whether a pulmonary hydatid cyst should be operated on at all. Often infestation was present elsewhere. Efforts should be made to exclude hydatid disease in the liver. The second point to decide was which side to attack first; he suggested the larger cyst first—the right—to allow a safer margin with more efficient aeration on the other side. The third point was whether one should perform a one-stage or two-stage operation; the latter course made the procedure simple and safe.

DR. ERIC PRICE commented on the hip condition. He said it was usually possible to succeed in reducing one dislocated hip. If two hips were dislocated there was nearly always difficulty. The explanation rested, not with the nature of the operation, nor with the difficult age at which this was attempted; there was more than a simple absence of the head of the femur from the acetabulum. Some deformity was present, and failure to reduce the dislocation was due to soft tissues, unrevealed by the X rays, which prevented reduction. In cases dealt with early, one should make an attempt at reduction by manipulation. In practice, this was done under the fluoroscopic screen and reviewed later to see what had gone wrong. Later one tried again with traction. One might achieve a position of the head opposite but not in the acetabulum. If the heads were placed in the optimum position and left for six weeks, their position might be rectified. If all these measures were unsuccessful, there was probably no hole to take them, or the acetabulum was too shallow. The conclusion was that reconstruction was necessary. This was not without its difficulties. There was a great tendency for such hips to become stiff. The best age for this procedure, according to Fairbanks, was between three and five years. The American school held similar views, and its adherents might operate after one failure only. One was advised not to subject these children to such measures at a later age, as a stiff hip was likely to result.

Dr. Forster, in reply, said that he was impelled to attack the cyst in the right lung first for the reason enunciated by Dr. Whitaker. With regard to the hips, Dr. Forster said that at operation he sometimes found nothing into which the femoral head could be put. In his experience, a great deal of deformity of the head followed repeated manipulations. For this reason, he was inclined to operate earlier than usual. However, he might be going through an evolutionary phase. He intended to show the child again at a subsequent meeting of the society.

Achondroplasia.

DR. ROBERT SOUTHBY showed a female patient, aged three months, suffering from achondroplasia; the condition had been recognized at birth. Dr. Southby drew attention to the large head, the trident hands, the normal body and the

short limbs. The baby also had a cleft palate, and Dr. Southby wondered whether this should be repaired at the usual time. Dr. Southby said that alternative names for the condition were *chondrodystrophia fœtalis* and micromelia. In 1878, Parrot had separated achondroplasia from fœtal rickets and cretinism. In 1892, Kauffmann had described the changes occurring in the cartilage. The anomaly was due to some interference with endochondral ossification, especially at the extremities of the long bones. Achondroplasia was almost always congenital, but in some cases it was said to have begun after birth. It might be handed down from one generation to another through the males, but it might also be familial. Twins had been known, one of which was achondroplasic and the other normal, and other twins, both of whom were achondroplasic. Some animals were regarded as examples of achondroplasia fixed by inheritance, such as the dachshund and acon sheep. Dr. Southby said the causation was unknown, but the condition was apparently brought about by some interference with early endochondral ossification between the third and the sixth months of intrauterine life. The most prominent features were the large head and the short limbs set on a body of normal size. The long bones, humerus, femur, tibia and ulna, were shortened, sometimes to about half their normal length. The shafts were firm and had prominent ridges for the attachment of muscles. The normal curves of the bones were symmetrically exaggerated, often giving rise to an "hour glass" appearance. Pronounced beading of the ribs and a small, narrow chest were present. The skull was enlarged, with some degree of hydrocephalus, widely open fontanelles and frontal and parietal bossing. The nasal bridge was depressed. The clavicles were normal. Microscopic examination disclosed aplasia of chondral ossification. Most children suffering from achondroplasia were still-born or died in early infancy. There was a pronounced contrast between the stunted limbs and the trunk, which was normal in length, but narrow. The hands might not extend below the greater trochanters. Achondroplasics could often, when standing, stoop and kiss their toes, or rise from the lying position without help from the arms. Shortening of the limbs was greater in the proximal segment (arms and thighs) than in the distal segment (forearm and leg); this was known as rhizomelic micromelia. These subjects presented furrows round the limbs, as if the soft parts were too large for the bones; the buttocks were prominent. They had lordosis, a protuberant abdomen and a waddling gait, and were late in walking. The head of the fibula was much nearer the knee joint than normal. The hands were trident-shaped. Dentition was normal, and generally the mental condition was well up to average. Secondary sexual characters developed normally. The umbilicus was below the middle point of the body owing to the short lower limbs. If the achondroplasic survived the first year of extrauterine life, the disease did not shorten life, except in the case of pregnant women. The height usually did not exceed four and a half feet. Achondroplasia might be associated with various other abnormalities.

Dr. Southby showed photographs of the baby taken soon after birth, and went on to demonstrate by photographs the distinguishing features of other types of dwarfism, including mongolism, atelosis, rickets, renal dwarfism, cœliac rickets, cretinism and gargoylism.

DR. KEITH HALLAM said that the anomaly apparently occurred early in intrauterine life and was a deviation from normal endochondral ossification. It was characterized by stunting of the long bones as distinct from *osteogenesis imperfecta*. Dr. Hallam wished to draw attention to a few interesting points about the skeleton. Firstly, the patients were referred to as lordotic, and no doubt they were in the juvenile and adult stages; but in the newborn, kyphosis was present in the dorso-lumbar region. Lordosis made its appearance with walking. Secondly, broadening and flattening of the bodies of the vertebræ were demonstrable in many cases. Thirdly, there was a difference in size between the tibia and fibula at birth, the tibia being shorter. Dr. Hallam said he was not familiar with what occurred later in this respect. Fourthly, no changes occurred at all in the epiphyses; ossification appeared there at the normal times. Lastly, inspection of the skull revealed dystrophy of the bones developing in cartilage, but not of those developing in membrane. Compensatory bulging could be observed where membrane bones occurred in the skull.

DR. BRUCE HALLOWS said that an achondroplasic was under treatment in the out-patient department of the hospital. The child had been brought because of extreme kyphosis, and the condition had improved with treatment on the "hammock splint" elaborated originally by Dr. Eric Price.

DR. WILFRED FORSTER thought that the mother's appearance was suspicious. She was of short stature, and her hands were trident-shaped.

Dr. Southby, in reply, said that he was grateful for the comments. He added that if a female achondroplasic became pregnant, Cæsarean section was necessary. If she had offspring, the baby was likely to be similarly affected. Frequently also an achondroplasic father begot an achondroplasic child. Dr. Southby said that in the photographs of the dwarfs shown for comparison, attention was drawn to the fact that all subjects presenting achondroplasia, gargoylism and mongolism respectively were like peas in a pod. Since cretinism was due to thyreoid lack, one could not help thinking that the other conditions would eventually prove to be due to some glandular or other type of deficiency. Dr. Southby said that he felt that mongolism was probably due to a suprarenal disturbance, but there was insufficient evidence to back any statement on the subject.

Correspondence.

HOUSING IN AUSTRALIA AS A POST-WAR PROBLEM.

SIR: Recent Press reports prompted me to obtain a copy of your November 27 journal and read the article "Housing in Australia as a Post-War Problem". Your journal is to be congratulated on the statements made in this excellent article.

Both architects and the Building Industry Congress have been hammering away at this housing problem for years, but their efforts are under a big handicap inasmuch as they can be called interested parties, and for this reason unfortunately much of what they say is looked upon with a degree of suspicion.

The medical profession is surely the one outstanding body to give the lead in this vital post-war problem. Who better than the medical man can say what effect poor housing is having on our Australian race?

Dr. J. H. Cahill, in his recent presidential address in Melbourne, presented an excellent case for housing in our city, and I have not the least doubt much of what he said could be applied to New South Wales. Dr. Cahill speaks with a first-hand knowledge of overseas housing. Two points from his address that make a strong appeal to architects is his insistence that the housing problem is far more than a matter of houses and his proposal that Australia would do well to seek the help of an experienced English housing man to say what should be our best approach to this vast programme of work.

Dr. Cahill's address I presume will be published in your journal. This will give medical men an opportunity to read his views, but to be productive of the good it deserves, I suggest it should be made available in a form of reprints to be circulated through other channels that will get it before the people and show the medical profession are interested in the welfare of the masses.

Yours, etc.,

40, Lonsdale Street, Melbourne,
December 8, 1943.

LESLIE M. PERROTT.

BRITISH PRESS DELEGATION AND MEDICAL HEALTH SERVICES.

SIR: Sir Neville Pearson suggests in his broadcast that national health schemes and services are acceptable to the British people. But it is my experience and opinion that any departure from the original meaning of the word "physician", and any variation from the original calling (in all its various departments) that the term physician (or doctor) implies, has significant and unfortunate results, in spite of all safeguards, affecting doctors and public alike.

The art of healing is a profession as well as being a science. It is this combination of profession and science that it is important to encourage and preserve. Any deterioration in this relationship results in misplaced and faulty responsibility in relation to the management, treatment, payment and authority, as far as the sick and injured are concerned.

There is also a diminished respect for, and deference to, scholarship and eminence; and there is a breakdown in the liaison and coordination between the related arts and sciences, since this relationship depends finally on goodwill and a natural refinement.

Yours, etc.,

J. L. WHITWORTH.

Kalorama,
Victoria,
November 19, 1943.

IN HONOUR OF SIR JOHN RAMSAY.

SIR: A citizen of Launceston has written to the public Press suggesting that a permanent memorial be raised to Sir John Ramsay during his lifetime. Sir John has just completed fifty years of professional life. He further suggests that the memorial should take the form of a medical library. Sir John has been approached and is favourable to this suggestion. A public committee has been formed and contributions are being asked for from the public. Sir John Ramsay's family is heading the list with a very substantial sum. The purpose of writing this letter is to let members of the profession know that if they care to make contributions of journals and books that these contributions will be very gratefully received. The great disability from which medical practitioners in Tasmania suffer, as compared with their colleagues on the mainland, is the lack of a large reference library. At the present moment it is necessary to send to the mainland for books that are out of the ordinary. The books that are specially asked for are those which contain matter of permanent and outstanding interest. The journals required are good runs of standard journals.

Everyone who knows the history of surgery in Australia knows of Sir John Ramsay's great contribution to it. He came to the Launceston General Hospital as a house surgeon in 1896, and from 1898 to 1912 was surgeon superintendent. During that period he published a large number of articles on surgical matters, many of which had a great influence on the surgery of the day. A full bibliography is below.

Anybody wishing to contribute is asked to communicate with the undersigned. Postage and carriage on books or journals will be paid by the committee.

Yours, etc.,

C. CRAIG,
Surgeon-Superintendent, Launceston General Hospital.

1. "Intravenous Injection of Normal Saline Solution in a Severe Case of Typhoid with Hæmorrhage: Recovery", *Intercolonial Medical Journal of Australasia*, Volume III, 1898.
2. "Treatment of Hydatid by a Modification of the Closed Method, with Illustrative Cases", paper read at Intercolonial Medical Congress of Australasia, Brisbane, September, 1899.
3. "A Case of Sub-dural Hæmorrhage, with Convulsions: Trephining, Recovery", published in *Intercolonial Medical Journal*, October 20, 1900.
4. "Two Cases of Gall-Stones Treated by Operation", published in *Intercolonial Medical Journal*, December 20, 1900.
5. "The Results of Trendelenburg's Operation for Varicose Veins in Fifty-Seven Lower Extremities", *Intercolonial Medical Journal*, April 20, 1901.
6. "Avulsion of Scalp", *Intercolonial Medical Journal*, April 20, 1901.
7. "An Obstinate Surgical Case Treated by Koch's Tuberculin", *Intercolonial Medical Journal*, July 20, 1901.
8. "Renal Calculus of Unusual Size Removed by Nephrectomy", *Intercolonial Medical Journal*, Volume VII, 1902.
9. "Sarcoma of Kidney Removed by Transperitoneal Nephrectomy", *Intercolonial Medical Journal*, Volume VII, 1902.
10. "Secondary Hæmorrhage from the Right External Iliac Artery following Perityphilitic Abscess", *The Lancet*, Volume I, 1903.
11. "Amputation of the Knee Joint under Spinal Anæsthesia with Eucaine", *Intercolonial Medical Journal*, Volume X, 1905.
12. "Prostatectomy", *Intercolonial Medical Journal*, June 20, 1905.
13. "Eosinophilia in Hydatid Disease", *Intercolonial Medical Journal*, July 20, 1906.
14. "Local Anæsthesia", *Intercolonial Medical Journal*, August 20, 1906.
16. "Apparent Death from Primary Heart Failure Under General Anæsthesia: Resuscitation by Means of Compression of the Heart through an Epigastric Incision", *Intercolonial Medical Journal*, September 20, 1906.
17. "Prostatectomy: Its Indication and Results", *Intercolonial Medical Journal*, May 20, 1908.
18. "Discussion on Relations of the Medical Profession to Public Hospitals", Australasian Medical Congress, October, 1908.
19. "Paper on the Surgery of Non-malignant Diseases of the Stomach", Australasian Medical Congress, October, 1908.
20. "An Apparatus for the Continuous Administration of Saline and other Fluids by Rectum", *Australian Medical Journal*, March 20, 1909.
21. "Unusual Cases of Hydatid Disease", *Australasian Medical Gazette*, June 21, 1913.
22. "Congenital Absence of the Right Femur", *Journal of the College of Surgeons of Australasia*, November, 1928.
23. "Special Article on Diagnosis of Appendicitis", THE MEDICAL JOURNAL OF AUSTRALIA, November 22, 1930.
24. "Hospital Problems", THE MEDICAL JOURNAL OF AUSTRALIA, Volume I, 1934.
25. "A Case of Human Pancreas Grafting in Diabetes" (unpublished).

Obituary.

JOHN ALEXANDER CAMERON.

WE are indebted to Dr. Mervyn Patterson for the following appreciation of the late Dr. John Alexander Cameron.

In the death of Dr. J. A. Cameron the profession has lost one of the senior general practitioners of Queensland.

A son of Donald Cameron, M.A., Headmaster of the Ipswich Grammar School, he received his university education at Cambridge, where he took his B.A. and later his medical degrees. After graduation he was a resident at Saint George's and Queen Charlotte Hospitals in London. He then started practice in Ipswich in 1896 in conjunction with the late Dr. W. N. Robertson and continued in practice there until ill health compelled him to retire in 1941.

Dr. J. A. Cameron was a man whose chief interest and pleasure in life was his practice; he seldom took a holiday, and even then was happier when he got back to his work again. He worked a heavy general practice in which his greater interest was in gynæcology and obstetrics, but he was a skilled general surgeon as well.

To my mind, his greatest virtue was his critical outlook on everything he undertook; he always questioned carefully before taking any measure in any way out of the usual. It was always an education to watch the meticulous care he exercised when operating, and he was equally painstaking when doing a panhysterectomy or a simple incision into an abscess.

As an obstetrician he was outstanding in his patience and in the excellence of his results. While a member of the Obstetrical Society of Queensland, he published "A Record of 2,000 Consecutive Cases of Midwifery", which was a document with complete notes of every case.

Dr. Cameron served for many years as a Council member of the Queensland Branch of the British Medical Association, of which he was a past president. He was also a vice-president of the obstetrical section in several congresses. He was a regular attendant at Branch meetings, although it necessitated a drive of twenty-five miles over roads that were far from pleasant. It was chiefly due to his endeavours that the Ipswich Clinical Society has been an active association for many years.

His widow, three sons and two daughters survive him. His eldest son, Donald, is a graduate in medicine of Sydney and is at present serving with the Australian Army Medical Corps in New Guinea. His other two sons are also serving with the Australian Army Medical Corps.

In his spare time J. A. Cameron was an omnivorous reader. He was a keen worker for Saint Paul's Church of England all his life, and for many years occupied the position of Chairman of Trustees of the Grammar School, and in his later years he was a keen Rotarian.

He will be greatly missed by his fellow practitioners in Ipswich, all of whom owe him much for his kindness and assistance which was available to them on all occasions. Ipswich and the profession generally will be poorer for his passing.

Naval, Military and Air Force.

APPOINTMENTS.

THE undermentioned appointments, changes *et cetera* have been promulgated in the *Commonwealth of Australia Gazette*, Number 258, of December 9, 1943.

CITIZEN NAVAL FORCES OF THE COMMONWEALTH.

Royal Australian Naval Reserve.

Termination of Appointment.—The appointment of Cyril Ignatius Wilkinson as Surgeon Lieutenant is terminated, dated 14th November, 1943.

ROYAL AUSTRALIAN AIR FORCE.

Citizen Air Force: Medical Branch.

The probationary appointments of the following Flight Lieutenants are confirmed: J. B. Felstead (257109), J. L. Sinclair (257225), J. H. S. Waters (267226), J. B. Foster (257222), H. G. Hiller (256811), W. B. Macdonald (256798), G. A. Robbie (256865), H. R. Thomson (256874), V. W. Threlkeld (256829), V. G. Walker (256955), F. P. Champion de Crespigny (255171), L. C. G. Colville (256713), R. A. Hill (257085), A. C. Schwieger (257345), W. W. Rail (255174), N. J. Chamberlain (256869), J. D. Connellan (256866), R. J. Fleming (257137), J. R. Watt (257402), R. E. Wood (256797), J. F. Ziegler (257480), G. Sutherland (256391).

The probationary appointment of Pilot Officer D. Gilmour (423707) (Entomologist) is confirmed, and he is promoted to Flying Officer with effect from 22nd October, 1943.

CASUALTIES.

ACCORDING to the casualty list received on December 14, 1943, Captain N. C. Derkenne, A.A.M.C., Hamilton, is reported to have been placed on the "dangerously ill" list.

According to the casualty list received on December 14, 1943, Major J. S. Chalmers, A.A.M.C., Hobart, who was previously reported missing, is now reported to be a prisoner of war.

Australian Medical Board Proceedings.

TASMANIA.

THE undermentioned has been registered as a duly qualified practitioner:

Rutledge, Norman Heaydon, M.B., B.S., 1943 (Univ. Sydney), Royal Hobart Hospital.

Nominations and Elections.

THE undermentioned has applied for election as a member of the New South Wales Branch of the British Medical Association:

Higham, Noel Ronald Douglas, M.B., B.S., September, 1942 (Univ. Sydney), Maitland Hospital, West Maitland.

Books Received.

"A Study of Absenteeism among Women", by S. Wyatt, R. Marriott and D. E. R. Hughes; Medical Research Council, Industrial Health Research Board Emergency Report Number 4; 1943. London: His Majesty's Stationery Office. 9½" × 6", pp. 12. Price: 2d. net.

"Food Inspection Notes: A Handbook for Students", by H. Hill, F.R.San.I., F.S.I.A., A.M.I.S.E., and E. Dodsworth, M.R.San.I., M.S.I.A.; 1943. London: H. K. Lewis and Company, Limited. Crown 8vo, pp. 130. Price: 6s. net.

"Diseases of the Ear, Nose and Throat", by Douglas G. Carruthers, M.B., Ch.M. (Sydney), F.R.A.C.S.; 1943. Sydney: Angus and Robertson Limited. 8½" × 5½", pp. 405, with many illustrations. Price: 25s. net.

"The Essentials of Modern Surgery", edited by R. M. Handfield-Jones, M.C., M.S., F.R.C.S., and A. E. Porritt, M.A., M.Ch., F.R.C.S., Lieutenant-Colonel, R.A.M.C.; Second Edition; 1943. Edinburgh: E. and S. Livingstone. 9½" × 6", pp. 1220, with 624 illustrations, of which many are in colour. Price: 40s. net.

"Medical Clinics on Bone Diseases: A Text and Atlas", by I. Snapper, M.D.; 1943. New York: Interscience Publishers, Incorporated. 11" × 8½", pp. 232, with many illustrations. Price: $10.75.

"The Natural Development of the Child: A Guide for Parents, Teachers, Students and Others", by Agatha H. Bowley, Ph.D., with a foreword by D. R. MacCalman, M.D.; Second Edition; 1943. Edinburgh: E. and S. Livingstone. 7½" × 5", pp. 200, with illustrations. Price: 8s. 6d. net. Postage 5d.

"The Care of Tuberculosis in the Home", by James Maxwell, M.D. (London), F.R.C.P. (London); 1943. London: Hodder and Stoughton. 8½" × 5½", pp. 115, with four illustrations and two diagrams. Price: 7s. 6d. net.

"Advances in Internal Medicine", edited by J. Murray Steele, M.D., *et alii*; Volume I; 1942. New York: Interscience Publishers, Incorporated. 9" × 6", pp. 302, with illustrations and diagrams. Price: $4.50.

"Advances in Pediatrics", edited by Adolph G. de Sanctis, M.D., *et alii*; Volume I; 1943. 9" × 6", pp. 314, with illustrations and diagrams. Price: $4.50.

"Fractures and Joint Injuries", by R. Watson-Jones, B.Sc., M.Ch.Orth., F.R.C.S., 1943, Volume I, Third Edition. Edinburgh: E. and S Livingstone. 9½" × 6¾", pp. 418, with many illustrations, some in colour. Price: 75s. net.

Medical Appointments: Important Notice.

MEDICAL PRACTITIONERS are requested not to apply for any appointment mentioned below without having first communicated with the Honorary Secretary of the Branch concerned, or with the Medical Secretary of the British Medical Association, Tavistock Square, London, W.C.1.

New South Wales Branch (Honorary Secretary, 135, Macquarie Street, Sydney): Australian Natives' Association; Ashfield and District United Friendly Societies' Dispensary; Balmain United Friendly Societies' Dispensary; Leichhardt and Petersham United Friendly Societies' Dispensary; Manchester Unity Medical and Dispensing Institute, Oxford Street, Sydney; North Sydney Friendly Societies' Dispensary Limited; People's Prudential Assurance Company Limited; Phœnix Mutual Provident Society.

Victorian Branch (Honorary Secretary, Medical Society Hall, East Melbourne): Associated Medical Services Limited; all Institutes or Medical Dispensaries; Australian Prudential Association, Proprietary, Limited; Federated Mutual Medical Benefit Society; Mutual National Provident Club; National Provident Association; Hospital or other appointments outside Victoria.

Queensland Branch (Honorary Secretary, B.M.A. House, 225, Wickham Terrace, Brisbane, B.17): Brisbane Associated Friendly Societies' Medical Institute; Bundaberg Medical Institute. Members accepting LODGE appointments and those desiring to accept appointments to any COUNTRY HOSPITAL or position outside Australia are advised, in their own interests, to submit a copy of their Agreement to the Council before signing.

South Australian Branch (Honorary Secretary, 178, North Terrace, Adelaide): All Lodge appointments in South Australia; all Contract Practice appointments in South Australia.

Western Australian Branch (Honorary Secretary, 205, Saint George's Terrace, Perth): Wiluna Hospital; all Contract Practice appointments in Western Australia.

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